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A GROUP OF HYPOTHESES BEARING ON CLIMATIC CHANGES.[1]

WHILE the atmosphere is the most active of all geological agencies, it has received the least careful study from geologists. Its very activity destroys its relics almost as soon as formed and gives them peculiar evanescence. This has invited the neglect of geologists laudably prone to concentrate their attention upon agencies which have left enduring and unequivocal records. The atmospheric element in geological history bids fair to long remain obscured by elusive factors and uncertain interpretations. None the less it is an element of supreme importance and should be persistently attacked until it yields up its truths. This must be my excuse for offering a paper which, I am painfully aware, is very speculative in many of its parts.

All our attempts at the solution of climatic problems proceed on some conscious or *unconscious* assumption concerning the extent and nature of the atmosphere at the stage involved. These assumptions are too often unconscious and the conclusions reached command a confidence which might not be reposed in them if the underlying assumptions were frankly stated. It may not be unwholesome, therefore, to raise some radical doubts respecting current assumptions regarding the early stages of the

[1] Read before the British Association for the Advancement of Science at Toronto, August 20, 1897. For obvious reasons it was necessary to treat the many factors involved with extreme brevity and hence with some obscurity and much lack of adequate qualification. I have taken the liberty of adding some tables and other matter

atmosphere and to offer for trial a competitive hypothesis or group of hypotheses. To admit a competitive hypothesis to the working list is a concrete form of embodying a doubt respecting existing hypotheses and serves better than any abstract skepticism to keep alive the sources of doubt. I assume that the system of multiple working hypotheses is accepted as furnishing the most wholesome conditions for research, and that any additional hypothesis not in itself incredible will be welcomed.

If we compute the mass of the several constituents of the present atmosphere, and estimate the rate at which they are being consumed in alterations of the superficial rocks, we find that the carbon dioxide will last but a short period unless there be some source of supply. A group of careful estimates by different methods gives results ranging from five thousand to eighteen thousand years[1] with a weighted mean of about ten thousand years. Only the alteration of the crystalline terranes was admitted to the computations. The estimates assumed the degradation rates of current geological opinion. Granting these may be too high, and multiplying the results accordingly, it still appears that we are confronted by the early exhaustion of a vital factor of the atmosphere, if there be no compensating source of supply.

There is, however, an immediate source of compensation. The ocean is an atmosphere in storage. It is not improbable

and of slightly extending and modifying the treatment on some points, but it still remains merely synoptic. The treatment of the periodicity of Pleistocene atmospheric changes is especially incomplete, but this is only a particular case under a general hypothesis whose value does not necessarily hang upon this individual application.

I desire to add that most of the questions involved in the paper have been discussed with scientific friends and with the advanced graduate students of my classes during the past two years, and that I have received from them much valuable aid. Computations and quantitative estimates have been made by F. R. Moulton, H. L. Clarke, A. W. Whitney, J. P. Goode, H. F. Bain, Samuel Weidman, C. F. Tolman, Jr., N. M. Fenneman, and C. E. Siebenthal, which I desire to gratefully acknowledge. The main points of the paper were presented to the Geological Club of the University of Chicago, October 1896.

[1] Made by A. W. Whitney, H. Foster Bain, J. P. Goode, Samuel Weidman, C. F. Tolman, Jr., and the writer.

that every portion of it has once been a constituent of the atmosphere and may be again. In atmospheric studies it must be recognized as a potential atmosphere. According to the best data at command, the ocean holds in solution about eighteen times as much carbon dioxide as the atmosphere. But even this reserve supply if fully available leaves the perpetuity of atmospheric conditions congenial to life very short, viewed geologically. This threat of disaster is not, however, a scientific argument, whatever function it may have in awakening interest and neutralizing inherited prejudice.

A broad comparison between the atmosphere of Palæozoic times and that of Cenozoic times fails, I think, to give proof of any radical difference in the constitution of the two atmospheres. The magnolia flora in North Greenland in Tertiary times indicates a scarcely less wide distribution of warm climate than the life of the same region in Palæozoic times. Glaciation in northern Norway announced by Reusch and confirmed by Strahan, in times apparently just preceding the Palæozoic era, is as suggestive of atmospheric poverty as anything that introduces the Cenozoic times. The signs of glaciation at the close of the Palæozoic era in India, Australia, and South Africa, reaching within 20° of the equator, indicate a thermal depression even more marvelous than that which closed the Cenozoic era. The salt deposits of the middle latitudes in Palæozoic times, notably those of Michigan and New York, in areas where the great basins now overflow voluminously, seem to imply an aridity quite comparable to anything which has succeeded. The extensive terranes of hematite-stained rocks, contrasted with the limonite-stained terranes, while their interpretation is more problematic, make suggestions of concurrent import.

A comparison of early with later life, stripped of theoretical presumptions, does not seem to me clearly to imply any great difference in the content of carbon dioxide. Air-breathing life, to be sure, has left no certain record earlier than the middle Palæozoic, but these earliest forms afford no clear proof that they were determined by non-susceptibility to an excess of carbon

dioxide. The delay in the appearance of land life is sufficiently assignable to the obstacles to its evolution to make needless a theoretical appeal to a noxious condition of the atmosphere.

But if we compute the amount of carbon which has been extracted from the atmosphere in the production of the carbonates and the carbonaceous deposits, and restore this to the atmosphere, following a time-honored custom, we are led to the time-honored conception of an exceedingly extensive, dense, warm and moist atmosphere. The amount of carbon dioxide represented by the limestones and carbonaceous deposits has been variously estimated at twelve thousand to one hundred and fifty thousand times the present content of the atmosphere. My own estimates lead me to favor figures lying between twenty thousand and thirty thousand. These estimates do not go back of the Palæozoic series and leave an unknown factor to be added for the pre-Cambrian limestones and carbonaceous deposits. The amount of carbon extracted from the atmosphere since the introduction of air-breathing life is probably not less than 8000 or 10,000 times the amount now in the air. This forces the question whether this large amount of carbon dioxide or any major part of it was ever in the atmosphere at any one time.

The alternative is to assume that the atmosphere was originally less ample and has been fed as well as robbed during all the geological ages, its history being a struggle between enrichment and depletion. In some measure this is an accepted view, but it is part of the purpose of this paper to show that the way is open to freer hypothesis in this direction.

The current view of a vast original atmosphere is derived less perhaps from the computation of material extracted from it than from theoretical views regarding the origin and early history of the earth. There has been quite general assent to the nebular theory of the origin of the earth. Even where dissent from the gaseous features of this theory has been entertained there has been acquiescence in the doctrine of the earth's early molten condition and all that it implies. If the earth were in a thoroughly molten condition, there would seem at first thought but

scant ground for any dissent from the inference that the present hydrosphere was then mainly a part of the atmosphere. It does not rigorously follow, however, that this was so. Hypothesis may go so far as to attribute much of the subsequent ocean and atmosphere to vapors thrown out of the molten magma as it cooled and to vapors gathered from space since. But I venture to question the supposed original molten state. While making no claim to disprove it, I doubt whether it rests on sufficiently solid theoretical grounds to justify the assumptions so unhesitatingly built upon it. There is still some ground to doubt the nebular hypothesis and to entertain some of the various phases of the meteoroidal hypotheses. The nebular hypothesis correlates a wonderful array of remarkable facts and has gained a profound hold upon the convictions of the scientific world, yet some of its great pillars of support have recently weakened or have fallen away entirely. Of the 5000 known nebulæ to which we naturally look for analogy very few, if indeed any, strictly interpreted, exemplify in a clear and decisive manner the systematic annular evolution postulated by Laplace. The photographs of the nebula of Andromeda, that were hailed with such delight on their first appearance as exemplifying the Laplacean hypothesis, appear upon more critical study to support it only in vague and general terms, if indeed they lend it support at all. The Saturnian rings, the trite source of illustration and analogy, prove under the test of the spectroscope to be formed of discrete solid particles, and not of gas, and the investigations of Roche have put a new phase on their theory. While in their form they tally with the annular hypothesis they do not support its gaseous phase, if indeed they lend it any support at all. But our chief interest is not in the general theory, but in the special inferences drawn from it respecting the early stages of the earth. Let us assume the possible or, if you please, probable truthfulness of the nebular hypothesis so far as the separation of an earth-moon ring from the shrinking sun is concerned. Do the subsequent steps commonly postulated logically follow?

The vast radiating surface of such a ring, its attenuated

nature and the extremely high temperature necessary to maintain its refractory substances in a volatile condition combine to suggest its speedy passage from the vaporous to the Saturnian or discrete solid condition from loss of heat. It seems a severe tax upon probabilities to suppose that such a ring would remain in the gaseous condition during the long period of its aggregation into a spheroidal form.

But a graver source of doubt is found in the high molecular velocities of the gases under these conditions. Dr. Johnstone Stoney[1] and others have attempted to show that the attractive power of small planets is insufficient to control gases of the higher molecular velocities, especially aqueous vapor. To this is attributed the measurable absence of atmospheres on the satellites and small planets. An endeavor to apply a similar line of reasoning to the conditions of the early earth leads to such disquieting results that I may be justified in briefly sketching it.

Each celestial body has an attractive power sufficient to control molecules shot away from it at velocities below a certain limit. At these velocities the discharged molecules pursue elliptical paths and return to the starting point. At the limit of these velocities they pursue parabolic courses and never return. Hence arises the expression "parabolic velocity" to indicate the limital speed at which particles shot away from the body will not return. The parabolic velocity of the earth at its surface is about 6.9 miles (1118127^{cm}) per second. A molecule discharged from it at that speed or a greater one will not return to it. The parabolic velocity is but an expression of effective gravity and

[1] "On the Cause of the Absence of Hydrogen from the Earth's Atmosphere and of Air and Water from the Moon." Royal Dublin Society, 1892. Since this paper was put in type I have been permitted to see an advanced copy of Dr. Stoney's later paper, "Of Atmospheres upon Planets and Satellites," Trans. Royal Dublin Society, Vol. VI, Part 13, Oct. 25, 1897, in which the author's investigations are much more fully set forth and his conclusions greatly strengthened. He takes account of the rotary speed of the outer equatorial zone and of westerly winds as projectile aids, factors which are neglected in this discussion. He also bases a very strong argument on the absence of helium from the present atmosphere, which on account of its chemical inertness would accumulate if it were not discharged.

depends not only upon the amount of the material embraced in the body, but on its distribution and other conditions. The parabolic velocity declines with height, as shown in the following table prepared for me by Mr. F. R. Moulton.[1]

I.

TABLE OF THE EARTH'S PARABOLIC VELOCITIES (v_p') FOR VARIOUS HEIGHTS ABOVE ITS CENTER (x) THE EFFECTS OF ROTATION BEING NEGLECTED.*

When x (height above center) $= 0$	$V_p' = +\infty$
When $x = r$ (earth's radius)	$V_p' = 11181.3$ meters
When $x = 7 \times 10^6$ meters	$V_p' = 10672.5$ "
When $x = 8 \times 10^6$ "	$V_p' = 9983.2$ "
When $x = 9 \times 10^6$ "	$V_p' = 9412.2$ "
When $x = 10^7$ "	$V_p' = 8914.1$ "
When $x = 12 \times 10^6$ "	$V_p' = 8151.2$ "
When $x = 14 \times 10^6$ "	$V_p' = 7546.6$ "
When $x = 17 \times 10^6$ "	$V_p' = 6848.4$ "
When $x = 20 \times 10^6$ "	$V_p' = 6313.9$ "
When $x = 25 \times 10^6$ "	$V_p' = 5647.3$ "
When $x = 30 \times 10^6$ "	$V_p' = 5155.3$ "
When $x = 40 \times 10^6$ "	$V_p' = 4464.6$ "
When $x = 60 \times 10^6$ "	$V_p' = 3645.3$ "
When $x = 10^8$ "	$V_p' = 2823.7$ "
When $x = 15 \times 10^7$ "	$V_p' = 2305.5$ "
When $x = 5 \times 10^8$ "	$V_p' = 1262.8$ "
When $x = 25 \times 10^8$ "	$V_p' = 564.7$ "

*$V_p' = \frac{\sqrt{2g r^2}}{\sqrt{x}}$ Log. $2g = 1.2923447$.

$2g = 64.32$ ft. $= 19.604$ meters. Log. $r^2 = 13.6092842$.

$r = 6{,}377{,}377$ meters. Log. $\sqrt{2g r^2} = 7.4508144$.

The parabolic velocity is also reduced by the centrifugal component of rotation. The effect of this is shown in the following tables computed by Mr. Moulton:

[1] Assistant in Astronomy at the University of Chicago.

II.

TABLE OF PARABOLIC VELOCITIES OF THE EARTH (V_p') FOR VARIOUS HEIGHTS ABOVE ITS CENTER (x) WHEN THE PERIOD OF ROTATION IS 23 HOURS, 56.067 MINUTES.*

When x (height above center) = 0			$V_p' = +\infty$
When $x = r$ (earth's radius)			$V_p' = 11181.27$ meters
When $x =$	7×10^6 meters	(4,349 miles)	$V_p' = 10672.49$ "
When $x =$	8×10^6 "	(4,972 ")	$V_p' = 9983.16$ "
When $x =$	9×10^6 "	(5,593 ")	$V_p' = 9412.15$ "
When $x =$	10×10^6 "	(6,214 ")	$V_p' = 8914.05$ "
When $x =$	12×10^6 "	(7,457 ")	$V_p' = 8151.14$ "
When $x =$	14×10^6 "	(10,700 ")	$V_p' = 7546.52$ "
When $x =$	17×10^6 "	(10,564 ")	$V_p' = 6848.31$ "
When $x =$	20×10^6 "	(12,428 ")	$V_p' = 6313.79$ "
When $x =$	25×10^6 "	(15,544 ")	$V_p' = 5647.17$ "
When $x =$	30×10^6 "	(18,643 ")	$V_p' = 5155.14$ "
When $x =$	40×10^6 "	(24,857 ")	$V_p' = 4464.39$ "
When $x =$	60×10^6 "	(37,286 ")	$V_p' = 3644.98$ "
When $x =$	100×10^6 "	(62,144 ")	$V_p' = 2823.17$ "
When $x =$	150×10^6 "	(93,216 ")	$V_p' = 2304.70$ "
When $x =$	500×10^6 "	(310,720 ")	$V_p' = 1260.14$ "
When $x =$	2500×10^6 "	(1,553,600 ")	$V_p' = 551.40$ "
When $x =$	10000×10^6 "	(6,214,400 ")	$V_p' = 229.19$ "
When $x =$	30434×10^6 "	(18,902,905 ")	$V_p' = .00$ "

*$V_p' = \frac{\sqrt{2gr^2}}{\sqrt{x}} - \frac{4\pi^2 x}{t^2}$ $\quad t = 23^h\ 56.067^m = 86164^s$.

Log. $\sqrt{2gr^2} = 7.4508144$. $\quad$ Log. $\frac{4\pi^2}{t^2} = \bar{9}.7257080$.

III.

TABLE OF PARABOLIC VELOCITIES OF THE EARTH (V_p) FOR VARIOUS HEIGHTS ABOVE ITS CENTER WHEN THE ROTATION PERIOD IS 1 HOUR, 24 MINUTES, AT WHICH THE CENTRIFUGAL COMPONENT OF ROTATION EQUALS THE ACCELERATION OF GRAVITY AT THE EQUATORIAL SURFACE.*

When x (height above center) = 0		$V_p' = +\infty$
When $x = r$ (earth's radius)		$V_p' = 11171.39$ meters
When $x =$	7×10^6 meters	$V_p' = 10661.69$ "
When $x =$	8×10^6 "	$V_p' = 9970.8$ "
When $x =$	9×10^6 "	$V_p' = 9398.2$ "

III—*continued.*

When $x =$	10×10^6	"	$V_p' =$ 8898,5 "
When $x =$	12×10^6	"	$V_p' =$ 8132.5 "
When $x =$	14×10^6	"	$V_p' =$ 7524.8 "
When $x =$	17×10^6	"	$V_p' =$ 6822.0 "
When $x =$	20×10^6	"	$V_p' =$ 6282.8 "
When $x =$	25×10^6	"	$V_p' =$ 5608.4 "
When $x =$	30×10^6	"	$V_p' =$ 5108.7 "
When $x =$	40×10^6	"	$V_p' =$ 4402.4 "
When $x =$	60×10^6	"	$V_p' =$ 3552.0 "
When $x =$	100×10^6	"	$V_p' =$ 2668.3 "
When $x =$	150×10^6	"	$V_p' =$ 2072.4 "
When $x =$	500×10^6	"	$V_p' =$ 485.7 "
When $x =$	691×10^6	"	$V_p' =$ .0 "

$$*V_p = \frac{\sqrt{2gr^2}}{\sqrt{x}} - \frac{4\pi^2 x}{t^2} \qquad \text{Log. } \sqrt{2gr^2} = 7.4508144$$

$$t = 1^h\ 24^m = 5040^s \qquad \text{Log. } \frac{4\pi^2}{t^2} = \bar{6}.1914987$$

The molecular velocities vary with temperature. The following table computed for me by Mr. A. W. Whitney exhibits these velocities for temperatures ranging from zero to 4000° C.:

IV.

TABLE OF AVERAGE MOLECULAR VELOCITIES FOR VARYING TEMPERATURES, IN CENTIMETERS PER SECOND, STANDARD PRESSURE.

	0°	100°	1000°	1250°	1500°	2000°	3000°	4000°
H_2	169611	198257	367258	400428	432243	489410	587282	671029
H_2O	56522	66067	122054	133501	144042	163093	195707	223619
CO_2	33259	38876	71819	78556	84759	95965	115160	131580
O_2	39155	45768	84551	92482	99786	112983	135576	154907
N_2	41735	48784	90122	98574	106359	120425	144508	165115

The molecules of a gas of a given temperature have a mean velocity, but this does not express the actual velocity of the individual molecules. By their interaction upon one another the velocities of some are depressed, the limit being zero, and the velocities of others are increased, the limit being infinity.

Theoretically both these limits may be reached, but extremely high velocities are acquired at such distant intervals as to be negligible. Very considerable exaltations of velocity are however attained with sufficient frequency to be effective in discharging a large part of the gas under suitable conditions, since each molecule in succession is liable to acquire a high velocity. The following table shows the proportion of molecules that reach or exceed the designated multiples of the average velocity at any instant:[1]

V.

TABLE SHOWING THE PROPORTION OF MOLECULES WHICH HAVE A GIVEN NUMBER OF TIMES THE AVERAGE 0°C. VELOCITY (OR MORE) AT ANY INSTANT, STANDARD PRESSURE, FOR TEMPERATURES RANGING FROM 0°C. TO 4000°C.

Proportion of Molecules	Times Average 0°C. Velocity for different Temperatures					
	t = 0°C.	t = 1000°C.	t = 1500°C.	t = 2000°C.	t = 3000°C.	t = 4000°C.
4.7×10^{-1}	1	2.2	2.5	2.9	3.5	3.9
1.7×10^{-2}	2	4.3	5.1	5.8	6.9	7.9
4.2×10^{-5}	3	6.5	7.6	8.7	10.4	11.9
7.4×10^{-9}	4	8.6	10.2	11.5	13.9	15.8
9.7×10^{-14}	5	10.8	12.7	14.4	17.3	19.8
9.6×10^{-20}	6	12.9	15.3	17.3	20.8	23.7
7.2×10^{-27}	7	15.1	17.8	20.2	24.2	27.7
4.2×10^{-35}	8	17.3	20.4	23.1	27.7	31.7
1.9×10^{-44}	9	19.4	22.9	25.9	31.2	35.6
6.5×10^{-55}	10	21.6	25.5	28.9	34.6	39.6

The molecules of water vapor at 0° C. have an average velocity of 56522^{cm} per sec. The foregoing table shows the

[1] This table was computed by means of the formulæ given by Risteen ("Molecules and the Molecular Theory of Matter," pp. 24–28), which are based on Maxwell's determinations. The high velocities assigned to a part of the molecules are mathematical deductions from data not altogether perfect, and are doubtless to be held with something less of firmness than would be warranted if they were experimental demonstrations, but in the absence of an available method of experimental demonstration these deductions may be accepted as the nearest approximation at present obtainable. A brief non-mathematical statement may be found in Maxwell's "Theory of Heat," pp. 314–316. The results require some modification for mixed gases and for special conditions, but this is not thought essential in this general argument.

number of times this velocity a given proportion of molecules attain at any instant when they have certain specified temperatures. For example, the table shows that when the gas is at 0° C., 47×10^{-1}, or 47 per cent. of the molecules have a velocity greater than the average velocity at zero centegrade; when the gas is at 1000° C., 47 per cent. of the molecules have a velocity 2.2 times the average velocity at 0° C.; when at 1500° C., the same per cent. have 2.5 times the average velocity at 0°C., etc. To raise the velocity of these molecules to the parabolic velocity of the earth the multiplier must be about 19.8 (since 1118127^{cm} per sec. is the parabolic velocity of the earth at the surface and $1118127 \div 56522 = 19.8$, nearly). The table shows that the proportion of molecules attaining this velocity or over (taking the figure nearest to 19.8) is as follows:

For 1000°	1.9×10^{-44}	For 3000°	7.58×10^{-20}
For 1500°	4.18×10^{-35}	For 4000°	9.7×10^{-14}
For 2000°	7.22×10^{-27}		

It now becomes important to ascertain how frequently all the molecules, on the average, will acquire the parabolic velocity of the earth. Every time a collision occurs the velocities of the colliding particles change. The formula for the time required for complete change will therefore be $\frac{1}{N\,Pm}$ where N is the number of collisions per second at 0° C. standard pressure, and Pm is the proportion of molecules having the parabolic velocity, given in terms of 0° C. velocity, standard pressure.

The number of collisions per second for 0° C. standard pressure is given by Maxwell as 17,750,000,000 for hydrogen, 7,646,000,000 for oxygen, and 9,720,000,000 for carbon dioxide. For the number of collisions for water vapor I find no authentic estimate, but it probably sustains the same ratio to the collisions of hydrogen and oxygen that their velocities do to each other, increased by a certain factor representing the effect of the size of the molecules. It will here be assumed that the number of collisions of the molecules of aqueous vapor is 10,000,000,000 per second at 0° C. standard pressure. The results can easily be modified for any other figure that may be thought nearer the

truth. The number of collisions also increases with the density of the gas. In the supposed case of an atmosphere containing all the water of the globe, the density would perhaps be 300 times the standard density. In the upper regions the density would be low and $\frac{1}{100}$ of the standard density may be taken as a representative of the conditions there. Making the assumption that the collisions of water vapor are 10^{10} per sec., the periods required for all the molecules, on an average, to acquire the parabolic velocity of the earth would be as follows:

Temperature	At Standard Pressure	At $\frac{1}{100}$ Standard Pressure	At 300 times Standard Pressure
1000°	1.7×10^{26} years	1.7×10^{28} years	5.7×10^{23} years
1500°	7.6×10^{16} "	7.6×10^{18} "	2.5×10^{14} "
2000°	4.3×10^{8} "	4.3×10^{10} "	1.4×10^{6} "
3000°	33 years	3300 years	40 days
4000°	1030 seconds	28.5 hours	3.4 seconds

Under the current hypothesis of a molten earth derived from a gaseous one the temperature of the atmosphere would probably exceed 4000° C. during the stages of condensation of the refractory material of the earth from the form of a gas to the form of a liquid. From this fervid stage the temperature would fall to 2000° C., or below before the crust would begin to form and the external liquid condition cease. The temperatures of a liquid earth may therefore be assumed to range from 4000° C. to 2000° C. or below, and the figures of the preceding table may be interpreted on this basis.

If the question were simply the acquisition of molecular velocities at the surface of the liquid mass greater than the parabolic velocity of the earth at that point within an available length of time, it would appear that the retention of the water vapor would be put in serious jeopardy during the hotter stages, but that it might survive the cooler ones in large part if it reached them, unless they were very prolonged. But there are other considerations to be taken into account. Under the most favorable conditions only a part of the molecules which attain a speed beyond the limital velocity of the earth's control would

escape because they would not be projected away from the earth. Besides, the escape of molecules projected outwards is seriously limited by the interference of the particles above. This interference is practically prohibitory for the molecules in the base of the atmosphere. The problem, therefore, involves the extent to which the high velocities of the lower hot atmosphere would be communicated to the upper atmosphere whence escape would be possible. The interpretation of this is beset with great difficulties. The molecular velocities of the higher parts of an atmosphere surrounding a molten earth involve factors which cannot be safely estimated from the phenomena of a cold earth. It must of course be assumed that the molecular velocities of the molecules of the rising air would be lowered in proportion to the work done by them or the energy lost, but in convectional movements certain parts of the air are recipients of motion rather than generators of it, and do not lose the energy their movements might seem to imply. It is probable that the interchange of lower and upper air about a molten earth would be extremely violent. It is not unlikely that explosive convection like that of the sun would be the customary mode of action. If hot bodies of vapor were shot violently into the outer limits of the atmosphere, molecular discharge would seem to be probable if not inevitable, whatever might be true of the more quiet mode of action. Besides this the current nebular hypothesis apparently involves the passage of even the outer atmosphere through very hot stages during the early period when the refractory gases of the now solid material were condensing and separating themselves from the atmospheric gases.

The case in this form seems at present indeterminate. There is an apparent probability that a large loss would be suffered while the temperatures ranged from 3000° to 4000°. At the same time there is a possibility that a residue would remain if the period of this high temperature were not prolonged, and a probability that a large part of the atmosphere would be retained if it survived until the temperatures were near the melting point of rock.

The considerations that grow out of altitude above the surface which reduces the parabolic velocity have been neglected thus far. These are not very important in a shallow atmosphere as may be seen by reference to the tables previously given, but they might be consequential in an exceedingly extended atmosphere. While it would be hazardous to estimate the height of a superheated atmosphere embracing the whole present hydrosphere, it seems not improbable that its outer portion would be appreciably affected by the reduction of the parabolic velocity due to its high altitude.

To this is to be added also the effect of the high rotation which the earth is assumed to have had. In the supposed discharge of the moon under either the nebular or fission hypothesis the attraction of the earth in the equatorial zone must have been nearly or quite neutralized by the centrifugal effect of rotation. This must have greatly promoted the expansion of the atmosphere in that zone and correspondingly reduced the earth's power to control its outer portion, indeed it is difficult to see how the moon could have separated from the earth without carrying away the atmosphere, unless indeed the separation took place while the material of both bodies was in a perfect gaseous condition and the atmospheric constituents were diffused throughout the entire gaseous mass. But even in this case the subsequent contraction of the earth should apparently have accelerated its rotation to such an extent that the retention of the outer equatorial atmosphere would be put in jeopardy.

There is still another consideration whose importance may possibly be decisive—the dissociation of water. Dr. Stoney has maintained that even under present conditions the earth is incompetent to retain hydrogen. This conclusion is in harmony with the fact that hydrogen does not permanently exist in the atmosphere, though this absence may be otherwise explained.[1] At 1000° C. all molecules of hydrogen would acquire the parabolic velocity of the earth some hundreds of thousands of times

[1] In his last paper, referred to in a previous footnote, the weakness of the argument from the absence of hydrogen owing to the ease with which it may combine

per second. Now the temperatures of the supposed molten earth reached and probably much exceeded the temperatures of effective dissociation of water vapor. The dissociation is probably due to violent impact of molecules of high velocities. It probably takes place in some degree even at moderate temperatures.[1] The proportion of dissociated molecules greatly increases with temperature until the dissociation so far exceeds the recombination that it may be said to be nearly or quite complete. Authorities differ as to the temperature of effective dissociation. The estimates commonly given lie in the lower half of the range of temperatures above assigned to the molten stage of the earth. If, therefore, the temperatures of the molten globe ranged as high as the current hypothesis seems to require, the dissociation of the aqueous vapor would seem to be inevitable and the loss of hydrogen would be endangered notwithstanding its disposition to recombine.

If the retention of the atmosphere be put in jeopardy by the earth's temperatures in a supposed liquid state much more would it be endangered if the temperatures were those of volatilization of the refractory material of the earth, as assumed by the Laplacean hypothesis, for not only would the molecular velocities be enormously increased, but the extension of the mass would push its exterior portions out into the regions of low parabolic velocity.

If the mass be still further dispersed into the vast gaseous ring of the Laplacean hypothesis the argument from molecular velocities is immeasurably strengthened, for not only must the temperatures requisite to the retention of the refractory substances of the earth in the attenuated condition of such a gaseous ring be exceedingly high, but the parabolic velocity of the body

with the free oxygen of the air is in a large measure covered by resting the argument chiefly on the absence of helium, which is chemically very inert. As helium is given off slowly by hot springs, it is urged that in the vast lapse of the geological ages it should have accumulated to an appreciable quantity if it had not escaped. As it has twice the molecular mass of hydrogen it is held that the "minimum speed of control" at existing temperatures lies below the molecular velocities of gases which are twice as heavy atomically as hydrogen.

[1] RISTEEN, Molecules and Molecular Theory, pp. 50–51.

in such an extremely distributive form would be exceedingly low. It would seem, therefore, that unless the argument from molecular velocities is radically and grievously at fault the hypothesis of a gaseous earth-moon ring is untenable unless a degree of tenuity be assumed which separates the molecules beyond the limits of effective kinetic relations. In this case the argument from rapid cooling becomes peculiarly strong and seems to leave no alternative but the conversion of the refractory matter of the ring into the discrete solid condition.

Impressed by these considerations and following what seem to be the legitimate implications of molecular studies, I have ventured for myself to place the atmospheric inferences from the supposed gaseous and molten conditions of the primitive earth in the list of uncertain deductions and to add an alternative hypothesis to my working list.

But occasion for doubt concerning an early molten earth and its vast atmosphere is not limited to this line of approach. On other grounds we cannot fail to recognize that some form of the meteoroidal hypotheses of the origin of the earth is entitled to be reckoned among the possibilities. Whether an accretion of meteoroidal matter would give rise to a molten earth or not would depend upon the rapidity and violence of the infall. If the intervals between falls were sufficient the heat would be lost concurrently. A relatively cold earth is theoretically as possible as a hot one until it is shown that the aggregation must be rapid. Even following the general line of the nebular hypothesis a cold earth is hypothetically possible. We have found reason for thinking that the earth-moon ring, if formed, would probably become cooled to discrete solid particles while still in the ring form. Now it does not appear that there are any conditions inherent in such a ring that tend toward sudden concentration into a spheroidal body. Quite on the other hand, the problem presented by such a ring is to find agencies which will lead to its concentration at all. Just how the concentration would take place is an unsolved question.[1] But two things seem certain; first,

[1] I have ventured to speculate a little upon this, though beyond the province of a

the process would be slow; the individual conjunctions more or less distant in time, and the heat generated by one impact so far forth lost before another took place; second, the conjunctions would not be opposing collisions but overtakes in which both bodies were moving at nearly the same speed, and the heat of conjunction hence relatively small. It would appear, therefore, that the aggregation might take place without the development *at any one time* of a general high temperature. The present accretions of the earth show us that growth is possible without notable increase of temperature. Following the general line of the nebular hypothesis, therefore, it is possible to suppose the earth to have been affected by relatively low surface temperatures at all stages of its growth. By changing our assumptions as to the rate and vigor of accretion we can correspondingly change our conclusions respecting the earth's temperature. The range

geologist, because it involves a supposed objection to meteoroidal aggregation. In a solid rotating ring the outer part moves faster than the inner and if broken and condensed to the globular form the rotation must be direct. But in a ring of planetoids the inner members move faster than the outer and if the several concentric orbits be symmetrically drawn together so that the inner planetoids uniformly or usually collide with the inner sides of the outer planetoids *retrograde* rotation follows. But this is inconsistent with the facts of the solar system except in the case of Uranus and Neptune (Cf. Faye, sur l'Origine du Monde, 1896, pp. 165, 270–281). But it seems improbable that this would be the mode of union except in the case of the outer planets, for the mutual gravitation of minute planetoids is very slight and expresses itself chiefly in perturbations under such conditions (see On the Stability of Motion of Saturn's Rings, Scientific Papers of James Clerk Maxwell, Vol. I, pp. 288–376), while the disturbing influence of the great planets is appreciable, as the ellipticity of the orbits of the planets testify. If the orbits of the particles or planetoids of the supposed earth-moon ring were at first nearly circular and concentric the conjoined attractions of the outer planets would render them elliptical. But the line of their apsides would not be concordant and would be subject to subsequent shifting in a more or less non-concordant fashion. It is therefore conceived that they would be brought to cross each other and that this would lead to collisions. Now an outer orbit could only cross an inner one by a more or less perihelion portion of it coming into coincidence with a more or less aphelion portion of the inner one. But the perihelion movement of a body in an outer orbit is greater than the coincident aphelion movement of a body in an inner orbit. Hence on the average the outer body in collision will have the greater speed and the consequent rotation will be direct. As this reasoning applies to the inner planets and not to the outer, and as the inner planets have direct rotations while the outer probably have retrograde rotations, it has at least the merit of coincidence with the facts.

of rational hypothesis seems therefore to be wide. It is herein urged that it is wholesome at present to recognize this wide range in its fullest amplitude.

But if we question current conceptions we should present alternatives which account for the atmosphere and for internal heat. Let us therefore hastily follow the hypothetical growth of a planet built up by the slow aggregation of small bodies which join it at low velocities and develop a minimum heat. Let the case be purposely made rather extreme to develop sharply the difficulties springing from it. Let the infalling particles be small and their rate such as not to generate a high surface temperature. The growth of such a body up to the size of the moon may be taken as an hypothesis of lunar history and the phenomena of the moon may serve as a check upon it. The moon may, however, have originated by fission even though the earth were built up by accretions. In the early stages of growth the gravity being low the aggregation may be supposed to have remained uncondensed. Volcanic aggregations of bombs, cinders and ashes are perhaps the nearest terrestrial analogues. The ingathering particles obviously carried with them so much of the atmospheric material as was entrapped or occluded within them in their solidification, or was absorbed into their pores or adhered to their surfaces. Judging from meteorites the amount of this might have been large. Gaseous molecules moving as independent bodies may have joined the aggregation and become absorbed in its porous body, but they would not have been collected into an appreciable atmospheric envelope until the body passed the size of the moon if the molecular considerations urged earlier in this paper hold good, though an atmospheric envelope would not have been entirely absent. As the mass grew the central pressure increased and condensation produced heat at the center proportional to the work done. I find the explanation of internal heat chiefly in this self-condensation, it being essentially the application of the Helmholtz solar theory to a solid body. Tidal kneading and chemical action doubtless added their contributions. When the growing mass reached the

size of the moon a definite problem was presented of which the present moon stands as a possible representative and invites computation. If in its loose state of aggregation the mass had a specific gravity of 2. and if it shrank by self-condensation to 3.4, the average specific gravity of the moon, the possible heat generated by the gravitative fall would have equaled 3900° C. for the whole mass, the specific heat being assumed to be .2, which is very prudent. I owe the computation to Mr. Moulton. For convenience of computation the condensation was assumed to be uniform and the distribution of heat uniform. The original distribution of internal heat would perhaps have varied with the square root of the pressure, according to Laplace's formula. As the computed temperature is more than twice the melting temperature of average rock not under pressure it seems ample for all igneous phenomena indicated on the moon with a large residue for secular loss.

Assuming that the exterior temperature remained below zero during the pre-atmospheric stages of growth, the hypothetical structure of the planet when it reached the size of the moon may be pictured as embracing (1) a dense central portion raised to a high temperature by compression, giving a potentiality of liquefaction under relief of pressure; (2) a zone of declining temperature and less compressed structure, graduating toward a porous condition, and (3) at the surface the still unconsolidated open aggregation. The low average specific gravity of the moon (3.4) encourages the belief that the outer porous zone was deep and open. The notion is entertained that the central heat and compression would lead to the expulsion of a part of the centrally entrapped gases and vapors, and that these would be driven outward into the exterior porous portion, which having a low temperature, like that of the moon today, condensed the aqueous vapor in the spaces of the open texture and the whole became bound together more or less completely with a matrix of frost and ice. It is assumed that the internal condensation would be attended by readjustments of matter of the nature of diffusions, differentiations and concentrations, and that there

would be deformations and igneous extrusions as on the earth today. Perhaps the reduction of metallic oxides and the working of the slag toward the surface may have been an incident of the process. Now whenever igneous extrusions invaded the zone of congealed vapor conditions would be afforded favorable for the generation of great quantities of steam temporarily restrained by the overlying fragmental mass and facily subject to explosive discharge. The peculiar constitution of such a body invites the notion of exceptionally explosive eruptions, as do also the extraordinary pits of the moon. As a matter of fact the suggestion arose from studying the pits and not from the peculiar constitution of the body to which the speculation had led. These remarkable cavities seem to be the close analogues of the few explosive craters which the surface of the earth presents.

The pre-atmospheric stage of the evolution would obviously cease when the growing earth acquired a size sufficient to measurably control its exhaled and ingathered gases. A certain measure of control was incidental to all stages, for even a small planetoid has some power to control gases of very low initial velocity if it continues at low temperatures. At the size of the moon gases of much more than the average molecular velocity of those of the earth at 0° C. could be held if their velocities were not exalted by interaction. This exaltation would become ineffective when the gases became extremely rare and the surface very cold. The molecular argument does not therefore affirm the total absence of an atmosphere on the moon, but rather on the contrary its scanty presence. An effective control would perhaps begin to be gained by the growing planet when the size of Mercury or thereabout was attained. After this the vapors and gases of lower molecular velocity would collect upon the surface and initiate the appreciable history of the external atmosphere. Whensoever the accretions of this atmosphere acquired the power of retaining the heat of the sun to such a degree as to give a surface temperature above the freezing point, the inauguration of the hydrosphere would take place and with its pro-

gressive development the familiar phenomena due to superficial waters would appear. The surface would soon lose its extremely fragmentary condition and take on the terrestrial form; the subterranean frozen zone would disappear and the vulcanism assume the terrestrial type.

This hypothesis, it will be observed, departs radically from the familiar view in that it initiates the atmospheric history by a tenuous envelope which continued to slowly increase. By the hypothesis, as thus far sketched, the atmosphere was derived from the interior. After the earth reached the requisite size the collection of wandering gases would supplement it. The competency of this external source is almost wholly a matter of conjecture and its vague possibilities need not be discussed here. It need only be remarked that the hypothesis of molecular discharge involves the peopling of space with flying molecules.

The measure of competency of the interior to supply an atmosphere is obviously a critical question. Unfortunately we are almost entirely without specific quantitative data bearing on the subject. We know that there is not a little atmospheric material in the interior as demonstrated in volcanic action and in the content of the minute pores of the hypogene rocks, but we do not know how far this was derived from the surface. If the moon never has had an appreciable external atmosphere its explosive eruptions were not due to surface infiltration and the implications of its numerous and vast craters are very suggestive. We can also draw inferences from meteorites which sometimes contain several times their volume of gases, as well as solid matter susceptible of conversion into atmospheric constituents. But at best we can only form very vague quantitative notions. On the other hand, we are liable to overestimate the amount required. The atmosphere and the ocean combined are little more than $\frac{1}{5000}$ of the mass of the earth. To be competent, the ingathered matter need therefore only contain about $\frac{1}{50}$ of 1 per cent. of atmospheric and aqueous material, plus an added factor for what may have been lost and what still remains in the interior. This percentage does not seem large enough

to render the hypothesis improbable in the present state of knowledge.

The competency of self compression to generate the internal heat of the earth is also a critical question already touched upon. Estimates made by different methods seem to give an ample supply. The safest seemingly is that of Mr. Moulton who simply computed the energy that would be required to lift the matter of a homogeneous earth of 5.6 sp. gr. against gravity alone to such a height as to give the whole a uniform specific gravity of 3.5. This is more than the present specific gravity of the moon and is obviously extremely conservative. The fall of this matter was found capable of raising the whole mass (specific heat being taken at the over-figure of .2), to 6560° C., or about four times the average melting point of rock at the surface. If the original specific gravity be taken at 2. on a gross average, which seems much more probable than 3.5, when the supposed loose state of aggregation is taken into account, the possible temperature, if all the potential energy were converted into heat and retained, would exceed 13,000°. A portion of the energy might take other forms than heat and a portion would be lost concurrently, but as the heat was generated in the interior and must have been conducted to the surface very slowly, the secular loss must have been of the conservative order. On the other hand, tidal friction and possibly chemical action would add to the interior heat and more or less offset these sources of loss. On the whole, therefore, self condensation seems a competent source of internal heat unless the rate of aggregation was excessively slow.

Although aside from my central purpose, it may be remarked that the recognition of a progressive self-condensation of the earth from a loose aggregation to a more dense one by a prolonged and still incomplete process presumes a degree and quality of shrinkage peculiarly suited to explain the inequalities of the earth's surface. An explanation must be found not only for the mountainous wrinklings of the crust in post-Cambrian times and the great crumplings and crushings of the Archæan

ages, so much neglected, but also for the great continental elevations and their superposed plateaus, and the deep oceanic depressions with their abyssmal anti-plateaus — phenomena with which current hypotheses have struggled so unsatisfactorily. It is also necessary to find an explanation for the unequal distribution of densities which have been partially revealed by gravity observations, but which are more broadly suggested by the unsymmetrical aggregation of the hydrosphere. The total shrinkage of the earth from first to last, under the hypothesis here proposed, would perhaps be sufficient to reduce its volume as much as one-half or even more, this, of course, depending on the original density. While the most of this contraction would antedate known geological history, the process can scarcely be supposed to have been complete in pre-Cambrian times, or even to be complete now. A part of the condensation must, therefore, quite certainly have fallen within geological history, and a part must remain yet to be accomplished, for, in addition to the retardation of the process of condensation caused by the heat generated, by the rigidity of the outer rocks and by the rapid rotation of the sphere, the maximum condensation of the mass could only be attained by means of a general rearrangement of the heterogeneous material of the meteor-built globe through the agency of diffusion, segregation, re-combination, re-crystallization and other processes which aid in giving a maximum compactness to mixed material. This internal readjustment must necessarily have been a slow process if the globe has been solid throughout its entire history, and must doubtless be yet incomplete. This progressive rearrangement of internal material adds a special agency of contraction to loss of heat, change of rotation and similar processes now recognized and which would act under this hypothesis essentially as under the current view.

If we make the plausible assumption that a slow process of diffusion, differentiation, concentration and gravitative readjustment has been in progress throughout the whole history and is yet active, and that matter has crept up from the hot compressed center into the superficial parts where relief of pressure would

cause liquidity, we seem to have an equally facile basis for the explanation of molten extravasations.

It may also be remarked that the acquisition of an atmosphere and hydrosphere at a moderate temperature when the growing earth reached a medial size introduced conditions congenial to life at a stage sufficiently anterior to the Cambrian period to satisfy the most strenuous demands of theoretical biology. Most of the restrictive arguments of Lord Kelvin and others lose their application under this hypothesis.

Returning to the atmospheric problem, it is to be remarked that the assumption of a limited early atmosphere may be entertained quite apart from the foregoing accretion hypothesis. Under the current hypotheses of the separation of the moon, whether by the annular mode of Laplace or the fission mode of George Darwin, great rotary speed and high temperature are assumed as necessary or probable conditions. We have seen that these seem to put the retention of the atmosphere in jeopardy. The balance of theoretical probabilities, as I now see them, favors the presumption that the atmosphere would have been greatly reduced under these conditions. There does not therefore seem to me any firm ground, even on current theories of the earth's origin, for insisting on the acceptance of the doctrine of a vast primitive atmosphere, as the great reservoir from which subsequent abstractions have been chiefly taken. I think we are free, therefore, to assume just such a Palæozoic atmosphere as the life and deposits of that time seem to imply, interpreted by the phenomena of today. Such an interpretation seems to me to indicate conditions not radically dissimilar to those of the recent geological ages; warm climates in high latitudes at times, colder climates in lower latitudes at times, moisture at times, aridity at times, and like oscillations. This view carries with it the necessary corollary that the atmosphere has been supplied by accessions in some near proportion to its losses. That additions have been made to the atmosphere of vital importance is a familiar doctrine, but it is here pressed to an unfamiliar degree.

If we push the doctrine thus far it is important to assign causes for the fluctuations of supply and exhaustion of the atmosphere, to give the doctrine a working form and to devise means of putting it to the test. Concerning external sources of enrichment we know so little that we can scarcely say that there is a leaning of probabilities either toward or against practical uniformity. The internal supplies were probably correlated in some measure with igneous extravasations — not that such extravasations were the sole mode of liberating gases, but that other modes probably worked concurrently with them. The escape of gases was probably also correlated with crustal movements, especially those that compromised the continuity of the surface rocks, particularly the profound crushings which mining and the microscopic study of the hypogene rocks reveal. In these phenomena therefore, may be found a rational basis for inferring the times of probable atmospheric enrichment. Formulated as a proposition, it may be postulated that special enrichment coincided with special igneous extravasation and crustal disruption, taking the earth as a whole. The supply may be assumed to have been uniform in so far as these and other means of liberation were on the average uniform.

The phases of depletion are susceptible of more satisfactory treatment. In the first place, the depletion was differential. The loss of nitrogen was doubtless slight, because of its chemical inertness, and hence, though the supply may have been small, the nitrogen grew to ultimate dominance. The depletion of oxygen through the alteration of surface rocks was notable, but less than that of carbon dioxide. As a result the latter became *the minimum factor of the atmosphere and the critical one*. The enormous reserve supplies of water rendered its consumption inconsequential.

In the second place, the depletion was conditioned upon the exposure of the surface rocks to atmospheric alteration. This in turn was conditioned upon topography. In stages of elevation the water table of the land is depressed and the zone of atmospheric penetration is deepened. At the same time the

zone of effective penetration of aerated water below this is also deepened. Hence the alteration of the rocks is promoted. In stages of low elevation — stages of baseleveling, for example — the zone of atmospheric penetration is shallow and rock alteration proceeds slowly. From this may be deduced the law that during stages of depression or baseleveling, depletion proceeded slowly. The aggregate surface must, of course, be considered.

To apply this law, let us assume for the moment, a uniform supply equal to the *average* rate of exhaustion. With the inauguration of any great epoch of general uplift there would begin an era of relatively rapid atmospheric exhaustion, which would proceed continuously during such elevated stage and might result in notable atmospheric impoverishment, as the computations cited early in this paper show. As the cutting down of the surface approached baselevel, the depletion would be retarded and, the supply continuing the same by hypothesis, the rate of exhaustion would fall below that of supply and an epoch of enrichment begin. A second elevation would re-inaugurate the depletion, and so oscillations of enrichment and impoverishment would follow the general oscillations of the land surface.[1] Applying this law by itself, atmospheric poverty should follow *at some distance* the stages of general elevation, and, on the other hand, atmospheric enrichment should follow at some distance the stages of baseleveling or depression.

But the assumption of a uniform average supply needs revision. In the main the igneous extrusions and crustal disruptions that are presumed to favor gaseous emanation probably fell in with the initiation of the elevated stages that favored depletion. In a general way the curves of supply and of depletion ran together in geological history and gave a measurably

[1] More strictly, the oscillations of that part of the land surface whose rocks consumed the atmosphere by their alteration — in general terms, the crystalline areas. Periodic general elevations followed by general baselevelings or some notable approach to baselevelings, are here assumed. It would be obligatory to state the grounds for this in an ampler discussion, but the all too narrow limits of this paper make this impracticable.

constant atmosphere, but their occasional failures to run in consonance are herein assigned as possible causes of exceptional climatic episodes, for it is almost axiomatic to say that climatic changes would attend changes in the constitution of the atmosphere. I assume that atmospheric poverty, especially in the critical item of carbon dioxide, is correlated with low temperature, as urged by Tyndall and others.

It is impossible here to attempt to apply the doctrine in detail to geological history. But it may be noted in passing that the Pleistocene glaciation followed at a notable interval the formation of the great plateaus and epeirogenic uplifts of late Tertiary times. The glaciation of India, Australia and South Africa occurred about the the time of the crustal revolutions that marked the close of the Palæozoic era. The uncertainty of the homotaxis of the strata involved makes a precise correlation at present impossible. The glaciation perhaps came too early to fit the hypothesis.[1] Here, at least, is an excellent chance to put it to trial. All other hypotheses of glaciation have fared badly when brought to the supremely severe test of the ancient oriental low-latitude glaciation, and if this hypothesis shall follow them to the junk shop of broken down theories it will find an already beaten path. The glaciation of northern Norway as determined by Reusch and Strahan succeeds the pre-Cambrian stage of elevation, but in what precise relations is not known.

The great extensions of warm climate to the high north appear to be associated with baseleveling periods in a general way ; but whether in a specific connection of sufficiently declared nature to indicate the relation of cause and effect remains to be determined.

Another source of atmospheric depletion needs recognition. Dr. S. W. Johnston is responsible for the opinion that the entire carbon dioxide of the atmosphere would be removed by the present annual growth of vegetation if there were no return through decomposition and animal life provided it were continued uni-

[1] It may fall under the organic factor of the hypothesis mentioned later.

formly for one hundred years.[1] Animal life, however, makes such nearly complete returns that the permanent loss is usually regarded as negligible. Nevertheless it is something. In certain stages of the world's history it has been important, as the coal beds testify. The loss in the Carboniferous age has been held sufficient to remove a noxious excess from the early atmosphere. On the same basis it might be held to cause serious depletion in the absence of the excess. It is necessary at least to consider whether, under the theory of a limited early atmosphere, conditions which restrain the animal factor of the organic cycle may not so far impoverish the air as to seriously affect climate. But this cannot be entered upon here. The organic cycle is very sensitive and very rapid in its action. It would naturally be greatly influenced by the topographic conditions which were concerned in the supply and exhaustion of the atmosphere, and lend to them either its concurrent or its counteracting influences.

It is now a little more than fifty years since Tyndall suggested that the periods of terrestrial glaciation might be dependent upon the carbon dioxide of the atmosphere whose peculiar competence to retain solar heat he had demonstrated. The suggestion of the origin of glaciation through the depletion of this atmospheric constituent is, therefore, not at all new. It has been entertained by others than Tyndall. If it has failed to find much acceptance this has perhaps been partly from a doubt as to its adequacy and partly from the lack of any definitely assignable cause for the requisite intermittent depletion. Dr. Arrhenius has recently contributed to the subject a most important discussion bearing especially upon the former point.[2] By an elaborate mathematical analysis of data derived from Langley's experiments he has endeavored to ascertain what degree of depletion of the carbon dioxide of the present atmosphere would bring on the conditions of Pleistocene glaciation, and, on the other hand, what degree of enrich-

[1] How Crops Feed, p. 47.

[2] Svente Arrhenius, Phil. Mag. S. 5, Vol. XLI, No. 251, April, 1896, pp. 237–279.

ment would produce the warm climate of the Tertiary. He arrives at the conclusion that the removal of 38 to 45 per cent. of the present carbon dioxide would bring on glaciation and that an increase of 2.5 or 3 times its value would produce the mild temperatures of the Tertiary times. He quotes the opinion of Professor Högbom in support of the competency of earth changes to produce this depletion, and also the competency of the interior and other sources to re-supply the impoverished atmosphere. He, therefore, carries the suggestion of Tyndall and others a very notable step in advance, and, what is especially important, has given it quantitative expression on the basis of deductions from observed data. He does not, however, postulate the conditions which control the enrichment and depletion of the atmosphere which has been the essential endeavor of this paper.[1]

But we do not meet geological demands when we simply offer *general* explanations of climatic changes. Our theories must ultimately be found to fit the precise phenomena. How are we to explain the profound glacial oscillations? Here is where existing hypotheses are put to the stress and our atmospheric hypothesis seems at first thought even less adaptable to the phenomena than most others. If we could deny that the oscillations were profound, as some do, it would be convenient. But I fear we cannot. We may appeal to variations of atmospheric supply, to the precession of the equinoxes, etc., but field experience leads me to doubt whether these will fully fit the phenomena, though they must doubtless be reckoned as factors. I have endeavored to follow out the doctrine of atmospheric gain and loss on its own lines, and although the studies are incomplete, the results are at least encouraging. I seem to find a rhythmical action that may in part explain the glacial oscillations. To do it justice it should have elaborate and careful statement, but I can here only suggest its nature in bald outline

[1] I may here remark that the main features of the ideas herein advanced were entertained and expressed to my students some time before I saw Dr. Arrhenius' important paper, but I fear I might not have felt justified in giving them a more public statement but for the encouragement of his weighty opinion on the vital point of quantitative sufficiency.

and in terms that need qualification. The idea hinges (1) on the action of the ocean as a reservoir of carbon dioxide and (2) on the losses of the organic cycle under the influence of cold. Cold water absorbs more carbon dioxide than warm water. As the atmosphere becomes impoverished and the temperature declines, the capacity of the ocean to take up carbon dioxide in solution increases. Instead, therefore, of resupplying the atmosphere in the stress of its impoverishment, the ocean withholds its carbon dioxide to a certain extent, and possibly even turns robber itself by greater absorption, though the diminution of the tension of the carbon dioxide of the atmosphere as its amount is reduced tends to increase the discharge of carbon dioxide from the ocean to restore the equilibrium, and, to the degree of its efficiency which is undetermined, offsets the increased absorption of the cold water. So also, with increased cold the process of organic decay becomes less active, a greater part of the vegetal and animal matter remains undecomposed, and its carbon is thereby locked up, and hence the loss of carbon dioxide through the organic cycle is increased. The impoverishment of the atmosphere is thus hastened and the epoch of cold is precipitated.

With the spread of glaciation the main crystalline areas, whose alteration is the chief source of depletion, become covered and frozen, and the abstraction of carbon dioxide by rock alteration is checked. The supply continuing the same, by hypothesis, reënrichment begins, and when it has sufficiently advanced warmth returns. With returning warmth, the ocean gives up its carbon dioxide more freely, the accumulated organic products decay and add their contribution of carbonic acid, and the reënrichment is accelerated and interglacial mildness hastened.

With the reëxposure of the crystalline areas, alteration of the rocks is renewed and depletion reëstablished and a new cycle inaugurated. And so the process is presumed to continue until a change in the general topographic conditions determines a cessation.

The rhythmic curve which represents these oscillations should have an increasing or declining amplitude, according to the advance or decline of the topographic conditions which control the depletion of the atmosphere. This brief sketch needs much elaboration and qualification, but as the studies are still in progress, and the paper has already transgressed the limits due the occasion, it must be deferred.

T. C. CHAMBERLIN.

AN ANALCITE-BASALT FROM COLORADO.[1]

THE discovery that analcite plays the rôle of an important primary constituent of certain igneous rocks must be regarded as one of the most interesting developments of recent petrographical investigations; and I, for one, am inclined to believe that Pirsson has not gone too far in his general conclusions, published in this JOURNAL a year ago,[2] that analcite is an essential, primary component of many rocks now assigned to the monchiquites, a rock group described some years ago by Hunter and Rosenbusch. As each definitely proven instance of primary analcite in igneous rocks must for some time to come be of value in establishing its true rank as a rock constituent, the following description is offered, although some important details of occurrence cannot be given.

The rock in question was found by myself in 1893, while engaged in the geological survey of the Pike's Peak quadrangle. The exact locality, which may be identified by reference to the published map of that area,[3] is in the small park called "The Basin," twelve miles nearly west of Cripple Creek. Near the southern end of The Basin, and on its western side, at the end of a little ridge between two branches of High Creek, there is an outcrop of black basaltic rock directly on the line where a great complex of andesitic and basaltic breccia and agglomerate rests on the Dakota Cretaceous sandstone. This volcanic series extends far to the westward, between South Park and the Arkansas River, but only a few tongues and remnants now exist to the eastward of The Basin. The outcrop mentioned was regarded

[1] Published with the permission of the Director of the U. S. Geological Survey.

[2] "The Monchiquites or Analcite Group of Igneous Rocks," by L. V. PIRSSON. JOUR. GEOL., Vol. IV, 1896, pp. 679-690.

[3] Geological Atlas of the United States, Pike's Peak Folio (No. 7), Washington, 1894.

at the time of discovery as indicating a short dike parallel to the ridge, but no contact was seen, owing to detritus. Although the rock had a somewhat unusual habit, it was not supposed to be materially different from many other basaltic dikes which had been observed cutting the fragmental series referred to. It turns out on examination, however, that this rock of The Basin is an analcite-basalt, quite unlike any other rock collected in the entire district.

The rock is dark and very fresh looking, with many small crystals of augite and olivine, and a white mineral occurring in roundish grains, all these averaging about 1^{mm} in size. A few augite prisms are larger, and terminations exhibiting the usual pinacoidal twinning were seen. The white mineral, by its rounded grains and the absence of cleavage, presents the only marked deviation from the normal habit of the neighboring plagioclase-basalts. A black, aphanitic groundmass holds the phenocrysts.

Under the microscope augite, olivine, and magnetite possess a development common in basalts. Augite of pale, yellowish green color, and very faint pleochroism, occurs in phenocrysts, which are usually almost free from inclusions, but a few aggregates of similarly colored grains are full of irregular or sack-shaped glass inclusions. There is commonly a narrow outer zone to the purer augite, in which inclusions of glass and magnetite are seen, and the substance of this outer rim has a faint purplish tinge, like that of the smaller groundmass grains. An analysis of the pure augite is given below.

Olivine appears in moderate abundance, of usual habit, and is the only mineral showing any sign of alteration. Perhaps half of the olivine is serpentinized with pale brown biotite leaves inclosed in the serpentine of the most altered grains, as an apparent secondary product. Primary dark, reddish brown biotite also appears very sparingly. Magnetite and apatite have the customary development.

A considerable part of the rock is colorless and isotropic in polarized light; a much smaller part is colorless but doubly refracting, and is mainly assignable to three species of feldspar.

The isotropic constituent embraces most of what is megascopically visible, and also much more, in small grains, which, with corresponding ones of augite, magnetite, and feldspar, make up the groundmass. There is, in fact, a regular gradation between large and small isotropic grains. No crystal form was observed for the isotropic substance, but neither does it appear in any way to play the rôle of a glassy base. It seems throughout to be an irregularly granular mineral constituent, of the isometric system.

The larger grains are almost wholly free from inclusions, and while probably the last substance to crystallize the isotropic mineral has in its growth pushed back the smaller grains of augite and magnetite so that they often form a distinct zone about it. This phenomenon seems clear evidence of a crystallizing force. The smaller grains mingle with augite, magnetite, and feldspar.

While no crystal form has been observed, rings or wreaths of small inclusions were noticed in a few grains, and these so strongly suggested leucite that until the chemical analysis was made I felt quite sure that the rock must be a leucite-basalt. A smoky tinge is present in a very few grains, and in one the coloring matter is arranged in zones, clearly suggesting a regular crystal form. Irregular fractures traverse the substance, and its index of refraction is less than that of the Canada balsam, as indicated by Becke's method.

The feldspathic constituent appears in small, irregular, clear particles, some of which have most characteristic microcline structure, with an extinction of 15°; others can only be considered as sanidine, with Carlsbad twinning in some grains; and the remainder is a plagioclase rich in soda, with very delicate albitic twinning. Its angle of extinction is always small. There is possibly some nepheline associated with the feldspar.

The purity, freshness and abundance of the isotropic mineral invited the attempt to determine its composition by isolation and analysis. This was done by Dr. W. F. Hillebrand, and the result is given in column I of the table below. Under I^a is given the molecular ratio deduced from the analysis. The

amount and composition of the portion of the rock soluble in hydrochloric acid is shown in column II. The augite was, in the main, so free from inclusions that an analysis was made by Dr. Hillebrand for comparison with other rock augites. The material was isolated by the Thoulet solution, and was found to be very pure on microscopical examination. III is the analysis of the augite.

	I	Ia	II	III
SiO_2	51.24	854	21.97	49.26
TiO_2				1.53
Al_2O_3	24.00	235	9.94[3]	6.01
Fe_2O_3	1.20[4]		3.71[4]	3.31
FeO				4.23
CaO	1.62	29	1.95	21.79
SrO	.06		.08	.06
BaO			?	?
MgO	.33	8	2.87	12.40
K_2O	1.25	13	.56	.41
Na_2O	11.61	187	4.04	.79
H_2O	.62[2]		3.91[5]	
H_2O	8.47[2]	470		
P_2O_5				
SO_3	none			
Cl	trace		.05	
	100.40		49.08	99.79

[1] Over H_2SO_4.
[2] Remainder of water.
[3] Includes P_2O_5.
[4] Total iron as Fe_2O_3.
[5] Assumed from rock analysis.

The isotropic mineral could not be secured entirely free from attached or included particles of doubly refracting substances. But these made up a small part of the material subjected to analysis. After treatment with hydrochloric acid there remained an insoluble residue, amounting to 4.22 per cent. of the substance taken, and this was found on microscopical examination to consist of augite and feldspar. From the molecular ratio the following proportion may be derived:

$$SiO_2 : Al_2O_3 : R_2O : H_2O :: 854 : 235 : 237 : 470 \text{ or}$$
$$3.6 : 1 : 1 : 2.$$

In this result CaO and MgO are united with the alkalis and iron

is neglected. This ratio is exactly that of analcite except for the silica. It cannot be assumed that the material isolated was pure, and small amounts of various substances no doubt went into solution. But we can reasonably consider it demonstrated that the isotropic substance acting like a mineral constituent has practically the composition of analcite. I can see no reason for doubting this identification.

The analysis of the soluble portion of the rock leads to practically the same result as that of the isolated analcite. By deducting olivine, apatite, and magnetite, there remains a residue having about the ratio 1 : 1 for Al_2O_3 to alkalis. Silica is again low. If nepheline were present in the rock the low silica of both analyses might be explained. It is possibly there in some small amount but probably not in sufficient quantity to entirely explain the figures of the analyses.

The augite proves to be quite high in alumina and to have more titanic acid than would be inferred from the pale violet tinge it exhibits. It is thoroughly normal augite.

An analysis of the rock of The Basin was also made by Dr. Hillebrand, and it is given in column IV of the following table, in which analyses of several allied rocks are introduced for purposes of comparison. Of the other analyses, V is of a rock from Shelburne Point, Vermont, described by J. F. Kemp and V. F. Marsters,[1] with other dikes of the Lake Champlain region. In the original description Kemp published another analysis of this rock, the accuracy of which he was afterward led to question, and the analysis here quoted, made by H. T. Vulté, was published by Pirsson, at Kemp's request, in his cited discussion of the monchiquite group. VI is of one of the original monchiquites from Brazil, by M. Hunter.[2] The relation between the analcite-basalt and the other basalts of the region is illustrated by analysis VII, by W. F. Hillebrand, of a normal plagioclase-basalt occurring in Saddle Mountain, a few miles northwest of The Basin.

[1] The trap dikes of the Lake Champlain region. Bull. 107, U. S. G. S., 1893 p. 32

[2] Über Monchiquit, ein camptonitisches Ganggestein aus der Gefolgschaft der Elæolithsyenite, Tschermak's Min. und petr. Mitth. Vol. XI, 1890, p. 445.

TABLE OF ROCK ANALYSES.

	IV.	V.	VI.	VII.
	Analcite-basalt, The Basin, Colo. W. F. Hillebrand.	Monchiquite, Shelburne Point, Vt. H. T. Vulté.	Monchiquite, Brazil. M. Hunter.	Plagioclase-basalt, Saddle Mt., Colo. W. F. Hillebrand.
SiO_2	45.59	45.58	46.48	48.76
TiO_2	1.32	undet.	.99	1.65
ZrO_2	.03	....	....	none
Al_2O_3	12.98	15.87	16.16	15.89
Fe_2O_3	4.97	4.65	6.17	6.04
FeO	4.70	6.37	6.09	4.56
MnO	.14	trace	....	.13
CaO	11.09	9.91	7.35	8.15
SrO	.12	....	....	.06
BaO	.13	....	....	.17
MgO	8.36	8.32	4.02	5.98
K_2O	1.04	1.61	3.08	2 93
Na_2O	4.53	3.42	5.85	3.43
Li_2O	trace	....	....	none
H_2O	.51	3.14	4.27	.40
H_2O	3.40			1.48
P_2O_5	.91	....	....	.60
Cl	.05	....	....	....
CO_2	....	....	.45	....
S	....	....	....	....
	99.87	98.87	100.91	100.23

Were the greater part of the water in the analcite-basalt deducted, and the remainder recalculated to 100, the analysis IV might stand for an ordinary nepheline-tephrite. It is lower in alumina than many analyses of basalt, but if TiO_2, P_2O_5, and MgO were accurately estimated in all such rocks the alumina would often fall 2 or 3 per cent. below the published figures. The magma of this basalt was relatively quite rich in soda with low silica and much water. The formation of analcite in the final stages of consolidation of such a magma seems to me much more natural than that glass should be the result, provided only that the conditions were in general favorable to crystallization. As the rock probably occurs in a dike and in a region where there are many dikes of holocrystalline plagioclase-basalt, the presumption must be that the conditions were favorable to crystallization.

The monchiquite of Shelburne Point agrees very closely in composition with this analcite-basalt. Its high alumina would no doubt be materially reduced if TiO_2 and P_2O_5 were deducted. Of this particular rock Kemp quotes Professor Rosenbusch, to whom it had been shown, as saying that it is a "true monchiquite of typical habit."[1] From a microscopical examination of a specimen of this rock which is in the reference collection of the Geological Survey it is plain that there is a great difference in the development of the two rocks. In the Shelburne Point rock there is a cloudy gray base of indistinct radially fibrous structure and of weak double refraction. It is neither glass nor normal analcite at present, whatever it may have been originally. It is clear from the analysis, however, that the rock of Shelburne Point and that of The Basin resulted from magmas of almost identical composition.

The original monchiquite (analysis VI) varies somewhat from the others, being higher in alumina, iron and alkalis and lower in lime and manganese. Its alumina must contain considerable phosphoric acid. Its isotropic base, interpreted by Hunter and Rosenbusch as glass, has been shown by Pirsson's recalculation of Hunter's analysis to have practically the composition of analcite.

The plagioclase-basalt of Saddle Mountain is so near to the monchiquite of analysis VI in chemical composition that if its magma had contained 2 per cent. more water it might in all probability have yielded a monchiquite. As many normal basaltic dikes occur near The Basin it seems reasonable to assume that the analcite-basalt magma contained more water at eruption than did those of the plagioclase-basalt dikes.

In the original description of monchiquite Messrs. Hunter and Rosenbusch call attention to the association of all the rocks of that type then known to them with elæolite-syenite, and express the belief that magmas of this kind must have given rise to the monchiquites together with some complementary acid rocks. The expression of this belief in the positive form of the title to

[1] Bull. 107, U. S. G. S., p. 35.

their communication—"Über Monchiquit, ein camptonitisches Ganggestein aus der Gefolgschaft der Elæolithsyenite"—is of course in harmony with the well-known attempts of Professor Rosenbusch to make geological occurrence the foundation stone in the classification of igneous rocks. And whether one believes or does not believe that the "dike rocks" of Rosenbusch have individually or collectively that restricted geological occurrence and that constant association indicating their origin which are assumed in the system of that master, it is of great importance to the development of petrography to know the facts. An association of rocks, the importance of which may be exaggerated from certain standpoints, should not on that account be slighted by those who occupy other points of view.

The analcite-basalt of The Basin occurs in a region where there is a great series of volcanic rocks, mainly andesitic, with basalts, trachytes, and rhyolites, all more or less prominent within five miles of The Basin. Phonolites occur in abundance at and near Cripple Creek, but there seems to be no reason for assuming any relation whatever between the magma of this analcite-basalt and that of the Cripple Creek center. As has been described[1] there are basic dikes at Cripple Creek, some of them plagioclase-basalts and some containing a scanty, colorless, residual material of indistinct character, which was interpreted as nepheline in large part, and hence these rocks were called nepheline-basalts. From the much decomposed condition of these dikes I am unable to say upon reëxamination that they may not originally have contained analcite or a glassy base, but still believe it more probable that they were nepheline rocks.

Since it is evident that the monchiquite or analcite-basalt magma contains nothing peculiar to itself except water, it is difficult to see why Rosenbusch should regard it as a differentiation product of different origin from other basaltic magmas. Nor is it plain why any significance, as to genetic relations, should be attached to the fact that the supposed glassy base of the mon-

[1] Geology of the Cripple Creek District, Colorado, by WHITMAN CROSS. Sixteenth Ann. Rep. U. S. Geological Survey, 1895.

chiquites is similar to elæolite-syenite in composition. The residual parts of any moderately basic rock, after crystallization of the ferro-magnesian constituents, will be identical with some possible extremely feldspathic rock.

As for the question whether the colorless isotropic base of the so-called monchiquites is really glass or analcite it must be admitted that both are possible, although the former has not been proven in any special case known to me. The point raised by Pirsson, however, seems very important, namely, that as the monchiquites are supposed to be rather deep-seated dike rocks, it is much more reasonable to suppose that the residual substance would crystallize rather than form a glass. This argument has special weight where it can be shown that the residue has practically the composition of analcite, and where associated rocks of the same or more silicious composition are found to be holocrystalline.

The name analcite-basalt has been used for the rock here described because it accurately expresses its relation to allied types, because the name has priority over monchiquite through its use by Lindgren[1] for the rock of the Highwood Mountains, Montana, and further because the definition of monchiquite by Rosenbusch implies a glassy base, which is certainly a possibility, so that there may be rocks to which the name monchiquite applies in the sense originally proposed. It is probable that in many cases it cannot be demonstrated whether the colorless isotropic residual matter is glass or analcite, and where decomposed it will be clearly a matter of inference, in most instances. Criteria will doubtless be discovered by which analcite can be more readily determined than at present. The advisable course then seems to be to apply the name analcite-basalt where the determination can be rendered probable and to apply monchiquite in other cases. The fact that an analcite-basalt would have been a monchiquite if its residue had not cystallized shows the extremely close relationship of the two rocks. But it does not follow that

[1] Eruptive rocks from Montana. Proc. Cal. Acad, Sci., Ser. 2, Vol. III, 1890, pp. 39-57 (reference).

all monchiquites would have yielded analcite-basalt on crystallization, for the ratio of $SiO_2 : Al_2O_3 : R_2O : H_2O$ must probably be very nearly 4 : 1 : 1 : 2 in order that this unusual rock constituent may form. With lower silica, nepheline and analcite, or glass, would presumably result.

WHITMAN CROSS.

STUDIES ON THE SO-CALLED PORPHYRITIC GNEISS OF NEW HAMPSHIRE.

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Introduction.—The following paper embodies the results of some weeks of field work on the New Hampshire terrane, heretofore considered by some writers to be a metamorphosed Archæan sediment, but suspected by others to be eruptive. The conclusions of the author corroborate this suspicion and he has attempted to express them here with the special point in mind. The author desires to express his best thanks to Professor J. E. Wolff, of Harvard University, for valued suggestions and very material aid during the progress of the work.

Historical summary of opinion on the "porphyritic gneiss."—From the conspicuous nature of its outcrops the "porphyritic gneiss" of New Hampshire early attracted the attention of geologists. In the first annual report of the Jackson survey, in 1841, Whitney and Williams, in describing its occurrence remark that "large bowlders of porphyritic granite are very numerous over the sur-

face, from the west parish of Concord to the center of Warner, where we find the rock itself in place. It is a peculiar rock, having large crystals of feldspar uniformly distributed through its mass; they are often glassy, so as to furnish beautiful and striking specimens. This bed of granite extends across the state in a general northeast and southwest direction. It is from eight to ten miles in width, though often interrupted with veins of granite of various texture."[1] With the physical difficulties of a rugged, forest-clad country, it was not to be expected that accurate determinations of boundaries could be made by these geological pioneers. To this fact is due the confusion of the "porphyritic gneiss" and associated schists in the published Portsmouth-Claremont section of the Final Report.

In their section, "from Haverhill to the White Mountains," Whitney and Williams again refer to the formation thus: "From Meredith to Centre Harbor the rock in place is porphyritic granite, often traversed by beds and veins of fine grained, dark colored granite and trap. Some specimens of the porphyritic granite, in which the crystals of feldspar are flesh colored, are very beautiful. . . . . "From Centre Harbor to Plymouth the rock in place is porphyritic granite, traversed by occasional beds of mica slate."[2]

The first distinct mention of the "porphyritic gneiss" in the second (and last) survey of the state, that under the control of Professor C. H. Hitchcock, occurs in the second annual report, 1870. The preliminary map issued with that report roughly outlines the formation which he calls porphyritic granite. He describes it as "common granite full of large crystals of feldspar, generally from one-half of one to two inches long, which gives a checked appearance to the ledges. Some portions of it have evidently been injected, while the arrangement of the feldspathic crystals in parallel lines leads to the suspicion of stratification in other cases. When accurately mapped the area will resemble the trunk and branches of a decayed tree, the

[1] Final Report on the Geol. of N. H., C. T. JACKSON, 1844, p. 51.

[2] Op. cit. pp. 73 and 137.

branches corresponding to the veins which have been injected from the original mass.[1]

In the report for the next year, Professor Hitchcock adopted the view which was held throughout the later publications of the survey. On the map, of a scale of five miles to one inch, he differentiates the Lake Winnipiseogee and White Mountain areas, and gives a brief description of the rock, to which he affixes the name "porphyritic gneiss." He says: "This is an ordinary gneiss, carrying numerous crystals of orthoclase or potash-feldspar, from a quarter of one to two inches long. The longer axes may be parallel to the strike or arranged helter-skelter. It passes into granite with the same porphyritic peculiarity of structure. . . . . We suppose this to be the oldest formation among the mountains. Geologists speak of a rock of this character as common in the Laurentian in various parts of North America and Europe."[2]

At the twenty-first meeting of the American Association for the Advancement of Science, held in 1872, Professor Hitchcock expressly referred the "prophyritic gneiss" to the Laurentian[3] and noted the common parallel structure of the rock which he concluded to be the trace of an almost obliterated stratification.[4]

An indication of doubt as to an exact correlation appears in Professor Hitchcock's "Classification of the Rocks of New Hampshire," published in 1873.[5] He divides the various forma-

[1] Second Ann. Rep. upon the Geol. and Mineralogy of N. H., 1870, p. 33.

[2] Geology of New Hampshire, 1874, Vol. I, p. 33.

[3] Explanation of a New Geological Map of New Hampshire. Proc. A. A. A. S., 1872, p. 134.

[4] Recent Geological Discoveries among the White Mountains, N. H. Proc. A. A. A. S., 1872, p. 135. In this paper the author states his grounds for the correlation, viz., that of lithological similarity between the porphyritic gneiss and the Laurentian of Canada and Europe. At each of the next two meetings of the Association, he reaffirmed his position that there is nothing older in the state than the porphyritic gneiss which was held to be Archæan in age. See Geological History of Lake Winnipiseogee. Proc. A. A. A. S., 1873, B. p. 122, and the Physical History of New Hampshire, *ibid.*, 1874, B. p. 76.

[5] Proc. Bos. Soc. Nat. Hist., Vol. XV., p. 304.

tions of the lower series into two groups — the Laurentian, which he qualifies by an interrogation point, and the Labradorian. The former included the "porphyritic gneiss," Bethlehem gneiss, White Mountain, or andalusite-gneiss, and the breccia of Franconia, in the order of decreasing age. The eight members of the Labradorian "constitute one horizontal series of formations, the lowest resting upon the upturned edges of all the parts of group I." The greater antiquity of the "porphyritic gneiss" than that of the other series "is inferred from the occurrence of several bands of andalusite and granitic gneisses upon both flanks." It is interesting to note that in discussion on this paper Dr. C. T. Jackson declared his belief that "the classification proposed was hypothetical to a great extent, and that sufficient reason for the adoption of the New York nomenclature was not shown."

The first volume of the Final Report of the Hitchcock Survey was issued in 1874. The "porphyritic gneiss" was there explained as the product of altered sediment, the primitive stratified rocks having been metamorphosed in Archæan, or, as then expressed, Eozoic time.[1] The second volume, published three years later, reiterated this opinion, making the terrane the representative of the "first territory in the state that was redeemed from the primeval ocean."[2]

"A porphyritic, or augen-gneiss, is eminently characteristic of the fundamental rocks in every part of the world, and hence ours may readily be called Laurentian."[3] A still closer correlation was suggested whereby the "porphyritic gneiss" of the White Mountain district, and inferentially that of the whole state, was put in the "upper division of the Laurentian system, as it is developed in Canada and New York."[4] In the general résumé of the stratigraphical relations, a thickness of five thousand feet was estimated for the formation. With it was included the "younger Bethlehem and Lake Winnipiseogee gneisses to form the whole Laurentian, aggregating 34,900 feet in thickness.[5]

[1] Geol. of N. H., Vol. I., 1874, p. 512.
[2] Geol. of N. H., Vol. II, 1877, p. 519.
[3] *Ibid.*, p. 668.
[4] *Ibid.*, p. 252.
[5] *Ibid.*, p. 668.

From this brief review it is seen that the placing of the "porphyritic gneiss" so low in the geological scale was largely due to the prevalence of two pernicious doctrines then held in the study of crystalline schists. The application of the "lithological canon" was a constant feature in the efforts of the second survey to work out their difficult field. The coarse granitic gneisses, the augen-gneiss, and the andalusite-gneiss were each supposed to be represented in the typical Laurentian of the better known regions, thereby establishing synchrony. Again the distinct foliation in many parts of the "porphyritic gneiss" led to the other serious error of considering the rock as a metamorphosed sediment, which still preserved traces of its original planes of stratification.[1] This position being taken, it was but natural to look for structural relations with the surrounding formations, and at many contacts, the greater antiquity of the porphyritic rock would often appear evident. Needless to say, however, in the light of present knowledge, that all such reasoning is without foundation so far as it refers to large isolated areas of thoroughly crystalline schists. Thus the character of the terrane had to be determined by other methods. The latter were very sparingly used by the survey, and consequently its final conclusions assigned to the "porphyritic gneiss" the very important position of a foundation member in the entire geological series.

The interpretation of other terranes was, of course, greatly influenced by this fundamental idea. The survey fixed the geological position of the Bethlehem gneiss,[2] and of the Montalban group and the Lake Winnipiseogee gneiss[3] directly by reference to the "porphyritic gneiss," and the later succession was correspondingly affected. In fact, Professor T. Sterry Hunt's conception of and nomenclature of the Montalban group was founded on the conclusion that there is this demonstrable Laurentian in New Hampshire.[4]

[1] Geol. of N. H., Vol. II, p. 99.

[2] 1871. See Geol. of N. H., Vol. I, p. 34; Vol. II, p. 452.

[3] *Ibid.*, Vol. II, pp. 564,662.

[4] Geol. Mag., 1887, p. 1499; Nature, Sept., 1888, p. 521.

We have already referred to some hints of an igneous, intrusive origin for the "porphyritic gneiss," that were given by the survey officers. Similar suggestions appear in many parts of the different reports.[1] The facts described in these passages were supposed to be explained on the metamorphic theory as being characteristic of only those parts of the ancient stratified rocks which had been altered to the extent of complete fusion.

In his address as vice president of section *E* of the American Association in 1883, Professor Hitchcock expressed some modification of his earlier opinions on the origin of the porphyritic gneiss. He said: "A careful study of the crystalline rocks of the Atlantic slope indicates the presence of scattered, ovoidal areas of Laurentian gneisses. Those best known have been described in the geology of New Hampshire. Instead of a few synclinal troughs filled to great depths with sediments, the oldest group is disposed in no less than twenty-two areas of small size, scattered like the islands in an archipelago.[2] There are no minerals in these Laurentian islands that do not occur in eruptive granite; and the schistose structure is often so faint that the field geologist need not be blamed if he acknowledges his inability to detect it. Likewise we discover the same fluidal inclusions and the vacuoles that pertain to granite."[3] Comparing these islands to volcanic oceanic islands of the present day, he suggests that the foliation of the porphyritic gneiss may be the result of the superposition in quaquaversal sheets of lava about each volcanic cone, aided by flows of mud and wear by water between igneous flows. In this way we might have a "concentric statiform arrangement in the whole mass." Subsequent metamorphism by heat and pressure would lead to the development of new minerals in foliated beds.

The next important notice of this formation appears in Whitney and Wadsworth's "Azoic system."[4] They instituted a close

[1] See Geol. of N. H., I, pp. 27, 33; II, pp. 102, 472, 520.

[2] Proc. A. A. A. S, 1883, E., p. 186.

[3] Op. cit., p. 187.

[4] Bull Mus. Comp. Zoöl., Harvard College, Vol. VII, 1884, p. 383 ff.

criticism of the general methods of the New Hampshire survey, and looked with especial disfavor upon the liberal use of the "lithological canon," as accepted by the members of that survey. A convenient résumé of the various classifications proposed for New Hampshire rocks is given in tabular form at page 396 of the memoir. The possibility of an eruptive origin for the "porphyritic gneiss" was remarked by the authors.

In 1884 Professor Hitchcock stated that "all thoroughly crystalline series of the Atlantic region are of Eozoic age."[1] Two years later he edited the "Geological Map of the United States," in which the "porphyritic gneiss" is colored as Laurentian.[2] A somewhat full account of his opinions on the formation was given in a paper on the "Significance of Oval Granitoid Areas in the Lower Laurentian,"[3] from which it is evident that in 1890 Professor Hitchcock held practically the same views on the present subject as those which he published in 1883.

While the present paper was in process of preparation the last word on the formation was given by Professor Hitchcock in this JOURNAL. After tracing in a general way the history of geological surveying in New Hampshire, he makes the following significant statement of a changed point of view: "The question now arises, how can our early classification [of the rock series] be improved? It is eighteen years since the New Hampshire report was published, and there are many new workers in the field, all placing great reliance upon petrographical principles, such as were inaugurated in Dr. Hawes' report. Some are advocates of extreme metamorphism, and hence the conclusions are not harmonious. It seems to us that our early views may be modified by the following principles: (1) The mineral characters of crystalline rocks are not a sure guide to geological age. (2) Protogenes, diabases, and diorites, more or less interstratified with hydro-micas, are of true igneous origin. (3) The

[1] Trans. Am. Inst. Min. Eng. XII, 1884, p. 68. Cf. a paper in Proc. A. A. A. S. of the same year (p. 396) where he again holds the porphyritic gneiss to be Laurentian.

[2] Trans. Am. Inst. Min. Eng., Vol. XV, 1886, p. 465.

[3] Bull. Geol. Soc. Am., I, 1890, p. 557.

Archæan gneisses and protogenes may also be of igneous origin, and their apparent stratification has no connection with sedimentary or chemical deposition, etc. . . . . Applying such principles to the classification of the rocks of northern New England, we may improve on the report in several particulars. (1) Archæan rocks are not eliminated from our list. They exist as oval areas, such as have been indicated in the Stamford gneiss, and south of Mount Killington, Vt., in the Hinsdale, Mass., area, the Hoosac Mountain, and elsewhere. I recognize the porphyritic gneiss in the Stamford rock, and in the Hoosac tunnel as Archæan. (2) Our hesitancy about the place of the Bethlehem gneiss is met by recent observations. They are batholites, containing inclusions of the adjacent mica-schists. It does not follow that all these protogene areas are of the same character; each one must be studied by itself."[1]

However satisfactory such conclusions may be in their appli cation to most of the formations in the state, a clear statement of the true relations of the porphyritic gneiss has not yet been made. The author's recognition of the correct methods which must be used in interpreting crystalline schists has as yet not been supplemented very largely by their positive exercise in the field, and the implication in the foregoing extract that the Archæan appears in the state in "oval areas," either igneous or non-igneous, is still without demonstration.

We shall hereafter adhere to the name porphyritic granite, for the rock under discussion instead of porphyritic gneiss which has been so far used. As will appear later, the former name, while not embodying all the generalized features of the rock, is preferable to the official one of the second geological survey.

Geographical distribution.—The porphyritic granite, as shown on the survey maps, occupies four large areas with several smaller ones. Of these the largest one extends from Mount Monadnock, N. 5° E. to the northern flank of Cardigan Mountain, a distance of sixty miles; while it varies from three to

[1] The Geology of New Hampshire. JOUR. GEOL., Chicago, Vol. IV, 1896, p. 57.

twelve miles in width. Since this occurrence covers more than four times as many square miles as any other, it may well be called the "Main area." Associated with it in Sullivan, Merrimack, and Hillsboro counties, are some half dozen much smaller outcrops of the same rock which are surrounded by schists, and are thus outliers from the larger mass. Twelve miles west by south of Mount Monadnock, a second important mass of regular elliptical or oval-shape cuts cross the Ashuelot division of the Boston and Maine railroad. The longer axis runs north and south, and is about ten miles long, while the shorter, transverse to the former, is six miles long. From the village of Ashuelot, situated on porphyritic granite, we shall derive a distinctive name, and call this the "Ashuelot area." The survey has mapped a large "White Mountain area," which is distributed in irregular elongated form at the north of the Main area from Mount Stinson to Mount Lafayette. Some twenty miles long, it also varies considerably in width, being only a mile wide near the Profile House, but broadening out to six miles at the Kinsman Notch. From there a long tongue of the rock runs southerly down the valley of the Pemigewasset River. The strike of this area is like that of the Main area, a few degrees east of north. Following the common axis of both areas northward from the Profile House, a small but important "Littleton area" of some eight or ten square miles in extent, appears near the town of Littleton. The fourth widespread occurrence of the porphyritic granite is found in another irregular mass thirty miles long, and from one to eight broad, running parallel to the Main area from Laconia to Waterville. This may be referred to as the "Winnipiseogee area," from its proximity to the beautiful lake of that name.

The very local outcroppings of this rock in other parts of the state are in point of size insignificant, but they are of value in helping to determine the relations of the whole formation. Notable among these are the small patch on the top of Mount Prospect west of Squam Lake and the long dikelike mass north of New Boston in Hillsboro county. The grounds for coloring in the mass of porphyritic granite at the southeast base of Mount

Monadnock did not appear to be corroborated by the present writer in a somewhat careful study of that region. The mantle of glacial drift is there very heavy, but the few outcrops which were discovered seemed to prove the bedrock to be of the same nature as the schists round about. Again, a visit to Mount Osceola showed that the area of porphyritic granite plotted on the survey map as occurring on its southern side is really occupied by the same coarse-grained hornblende-granite which occurs in the bed of Mad River, the "Conway Granite" of the survey.

So great being the extent of the formation, it was impossible in the time at the disposal of the writer to make a close examination of all parts of the porphyritic granite. Accordingly, most of the observations in the following pages refer to three areas, the study of which promised to be most fruitful in the problem before us. Those selected were the Winnipiseogee area, the Ashuelot area, and the contact of the Main area from the town of Jaffrey to Henniker on the Peterboro and Hillsboro branch of the Boston and Maine Railroad. We shall consider these areas separately, treating of the geological results obtained in each along with certain other facts whose arrangement would be difficult by any other method of discussion.

Brief description of the porphyritic granite.— General macroscopical descriptions of the granite are given in several parts of the "Geology of New Hampshire." The rock is remarkably simple in its phasal differentiation; so far as a considerable collection shows, there are only two important variations in it throughout its whole extent. It may be either a porphyritic granitite or a porphyritic granite proper, and either of them may have the foliated structure. They pass into each other by insensible gradation in all the areas and it seems to be impossible to map them separately. With the exception of this and a few other variable features noticed in the sequel, the porphyritic granite is essentially uniform; it is thus possible to dismiss its characterization in a comparatively summary manner. In a future paper, the author hopes to give an account of certain

petrographical features whose description here is not rendered either advisable or necessary for our present purpose.

The name "porphyritic granite" for this rock is regarded as the best available one both on the ground of inherent meaning

FIG. 1. — Photograph of a specimen of the porphyritic granite — obtained near New Hampton Centre — illustrating general habit of granitic phase, twinned phenocrysts, etc. — about natural size.

and of precedent. The composition and order of crystallization make it a true granite rather than a gneiss although the pseudo-schistose structure is so generally present. For very similar rocks, the names "granite-porphyry," "gneissic granite," etc.,

have been employed. It seems best to use the original name at first used by each of the New Hampshire surveys and in this we follow the nomenclature of Lehmann, Sederholm, Gümbel and others who have dealt with very similar rocks.

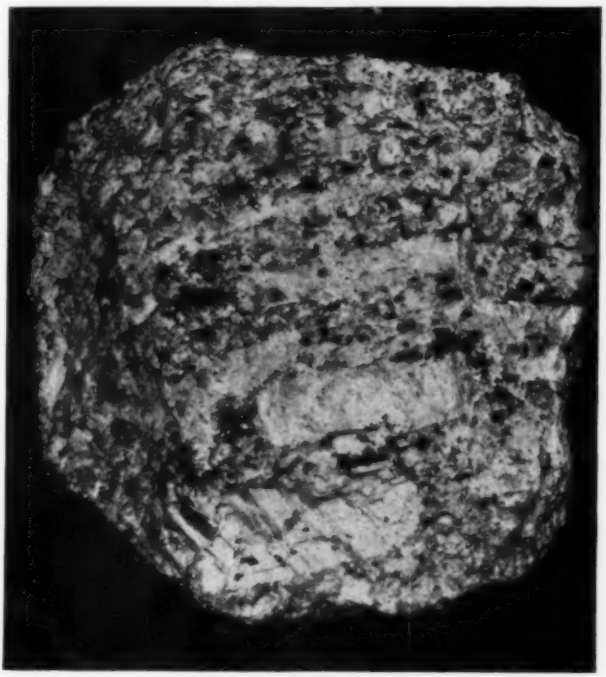


FIG. 2.— Photograph of a specimen of the porphyritic granite, showing a foliated phase near Hancock Station. The lowest phenocryst displays two prismatic partings besides the normal cleavages, parallel to *P* and *M*. The base is nearly in the plane of the paper. About one-half natural size.

The granite is always striking and handsome in appearance The groundmass is a coarse-grained, light to dark gray, granular aggregate of quartz and feldspar interspersed with flecks, blotches and lines of greenish muscovite and brilliant brown biotite.

Within are embedded the lustrous phenocrysts which generally, though not always, lie in the foliation plane when the rock has the plane-parallel structure.

The phenocrysts are glassy to opaque white and seem to be in every case either orthoclase or microcline, which may be intergrown with another (triclinic) feldspar in the usual microperthitic fashion. The largest ones are as much as twelve centimeters long. The usual habit is that of simple Carlsbad twins. They present straight edges to the surrounding matrix, giving the planes (001) (010) (110) and (101). This idiomorphic appearance is often lost in the slide through the great amount of resorption and marginal corrosion. Occasionally small shreds of biotite, a minute individual of apatite, or a few grains of quartz may appear in the core of the feldspar, but as a rule it is notably free from primary inclusions. The results of decomposition are normal. The changing to a reddish hue is common in some weathered phases.

The matrix of the rock is simply a typical coarse granitite on the one hand or a granite proper (in the classification of Rosenbusch) on the other, in either case with or without the foliated structure. The feldspars have the same general characters as those of the earlier generation except that a triclinic feldspar, probably andesine, now appears as an independent constituent. Quartz, biotite and muscovite constitute the other essentials. Large but relatively few individuals of magnetite, titanite, apatite and zircon are accessory. The quartz and feldspars are roughly equidimensional with diameters becoming as much as a centimeter in length; in fact the feldspars are often transitional into the phenocrysts. Quartz very often crystallized simultaneously with the feldspars resulting in the formation of a true micropegmatite which is extremely common in slides of the granite from all the areas. It is best shown in specimens collected along the railroad from Hinsdale to Ashuelot. There can be little doubt that the structure is primary in the formation as a whole. It is possible, however, that in zones of stress the development of the structure has been aided by the meta-

morphic cause of crushing, as described by Howitt, Hobbs and other writers.

The proportions of phenocrysts to matrix and of acid to basic constituents are quite constant in the porphyritic granite. The latter relation is that usually found in most highly acid granites. Now and then, a well-foliated segregational mass of biotite, quartz, much titanite and apatite, wrapping about phenocrystic cores of feldspar, may be encountered. Again, a phase of the rock nearly devoid of phenocrysts is not rare, although quantitatively it is insignificant as compared with the porphyritic phase. Both of these variations from the type will be discussed in what follows.

Field relations: The Winnipiseogee area.—The general distribution of the porphyritic granite in the Winnipiseogee area is described at length in "The Geology of New Hampshire."[1]

The topography of the area is, on the whole, not of a very definite nature. The greater reliefs, which vary from 800 to 1500 feet above sea level, are without distinct trends, and are the forms which might be expected as the result of eroding a massive rock of pronounced homogeneity. Upon this rolling ground the glacial drift has been deposited in unusual thickness, especially in the southern half of the area, where the hills are commonly composed of washed drift and till. Correlative with glacial reliefs are the glacial depressions seen in the numerous lakes and ponds which make such picturesque variety in the landscape of Carroll county. These modern deposits make it difficult to work out the relations of the bed-rocks. The variety of outcrops in many parts renders the determination of contact lines almost impossible, and it is largely to this cause that the suppositional nature of some of them is due.

The geological relations of the Winnipiseogee area.—The survey map places the porphyritic granite of this area in contact with the Lake Winnipiseogee gneiss, the Montalban group of schists, the Rockingham mica-schist, and the various eruptive masses of Waterville. For convenience we shall briefly indicate

[1] Vol. II, p. 592.

the facts of field observation which have been adduced in connection with each of these formations.

The porphyritic granite in contact with the Lake Winnipiseogee gneiss.—Close by Gilford Station, on Smith's Neck, the boundary line of the porphyritic granite and the Winnipiseogee gneiss appears, and, so far as known, this is its most southerly extension in this district. It is a typical acid biotite-gneiss at this place, elsewhere muscovite may be found. A few of the outcrops are significant. Here and there in the porphyritic granite masses of rock very similar to the main body of the schist occur in a horselike relation, although nowhere could the actual contact be found, and thus render possible a proof of that derivation of the masses. On Governor's Island, two miles to the westward, a large outcropping of a coarse muscovite-biotite-gneiss occurs. This is a typical representative of the Lake Winnipiseogee gneiss, and this mass is completely surrounded by porphyritic granite for a distance of at least a quarter of a mile in all directions. About that distance from this outcrop of schist, the real contact of the porphyritic granite and Lake Winnipiseogee gneiss was found, and it became evident that in the first occurrence we had to deal with an outlier removed by some means from the parent schist terrane. This relation could hardly be explained except on a hypothesis of the igneous intrusion of the coarser rock in which the schist was enclosed as a great horse. The truth of this supposition was strengthened by the discovery of two marked apophyses of the porphyritic granite running into the inclusion. Returning to the molar contact, a corroboration of our conclusion from a study of the outlier appears in a clearly defined tongue of the porphyritic granite which can be traced for some distance into the schist. Whereas on Smith's Neck the porphyritic granite was largely granitic, here a well-defined foliation characterizes the rock. The strike of the foliation planes is N. 3° E., and the dip is 82° to the east. It is noteworthy that the strike and dip of the schistosity in the adjacent schist is the same. In other words, there is an apparent conformity between them. At several places among the numerous outcrops of porphyritic gran-

ite on the island, very coarse pegmatitic dikes appear which are similar to the granite, and do not appear to belong to the class of pegmatite veins of segregational origin so common in the crystalline area of the state.

About a mile and three-quarters from Weirs there is a small inlet, on the south side of which the porphyritic granite is in contact with a dark coarse-grained gneiss. This seems to be equivalent to the Lake Winnipiseogee gneiss. It is cut by an apophysis of the porphyritic granite, and a horse of the schist can be seen on the bare ledges enclosed within the other rock. Again, as on Governor's Island, there is an apparent conformity of position.

Just across Meredith Bay, on Spindle point, an interesting contact occurs. The porphyritic granite outcrops occasionally on and about the road from Meredith with pretty definite foliation, the strike varying from N. 20° E. to N. 40° E., the dip high, but changing from easterly to westerly in an irregular fashion. On the top of the hill at which the road ends, an solated mass of another rock is conspicuously displayed in the well-smoothed ledges. It is dikelike in its form, being fully four hundred yards long and from ten to twenty wide. There is a distinct schistosity with its planes parallel to the longer axis of the mass which strikes S. 40° W. The dip is high at about 75° to the southeast, making an apparent conformity with the enclosing porphyritic granite, which is here well foliated. Within a distance of 300 yards south of this long band, and oriented with the longer axis parallel to it, smaller bodies of the same rock occur, again completely enclosed by porphyritic granite. They have the same pronounced structure-planes with a similar relation to the foliation of the country rock. From the hill top the largest of all these parallel bodies strikes toward a larger body of the same rock. Like the other, this mass is characterized by a strong gneissic structure. It also possesses an elongated form. We have here to deal with a number of horses immersed in a once molten magma which chrystallized out as porphyritic granite. The patent differences of grain and

structure, the sharp boundary between the two rocks, the irregular thinning and thickening along the length of the inclusions, all point to this belief. Here and there, within their boundaries, obscure apophysal extensions of the coarser rock may be observed. But this conclusion reaches practical certainty when the molar contact of the porphyritic granite and the "Lake Winnipiseogee gneiss" is studied. The latter is seen to be precisely the same coarse-grained biotite-gneiss as that in the inclusions, except for certain differences which can be explained as due to exomorphic change wrought by the porphyritic granite. Apophyses of the latter may be seen at the neck or isthmus of the peninsula cutting the gneiss. An important relation subsists between this line and the arrangement of horses on the hill. Not only are they parallel to one another, but they lie parallel to the line of molar contact.

Transition zone at Centre Harbor.—At Centre Harbor the gneiss lies on the east side of the boundary and at some distance from it is the usual coarse-grained muscovite-biotite rock of the Lake Winnipiseogee gneiss terrane. Its foliation planes strike N. 15° W., and the dip is about 60° easterly. As one approaches undoubted porphyritic granite through a distance of fifteen feet from the contact, one notes the large orthoclase and microcline feldspars two inches long, which normally are confined to the porphyritic granite, now scattered through the finer-grained rock with their longer axis parallel to its schistosity. They grow more numerous as the porphyritic granite is neared, until finally some four or five yards from their first appearance, the outcropping rock is typical porphyritic granite. It possesses the same strike and dip as the gneiss, being well foliated. There is thus a complete and slow graduation of the one terrane into the other, making it impossible to draw any line between them.

Transition zone near New Hampton Station.—A similar zone of transition between these two terranes occurs a mile and a quarter from New Hampton Station on the road running southeast from the station. The zone of passage is here much wider

than at Centre Harbor, but the properties of its magmalike gneiss with the sprinkling of large porphyritic crystals of feldspar are identical with those of the other locality. As a rule, there is a sharp contact between the porphyritic granite and the invaded rocks, like that which in general characterizes plutonic bodies. These belts of transition have at first sight a puzzling appearance. That they are, in reality, eruptive contacts seems, however, to be unquestionable. Durocher long ago noted such an intimate union along the boundary of gneiss cut by stock-granite. He considered the temperature of the igneous rock in such instances sufficiently high to produce a melting up of the gneiss the "particules" of which "ont dû posséder une assez grande mobilité, et cristalliser à peu près dans les mêmes conditions que les molécules du magma granitiques.[1] In his "Geog. Beschreibung Bayerns,"[2] Gümbel speaks of there being numerous transitions from "bunter gneiss" to "bunter granit" which cuts the former. Michel-Lévy has very clearly discussed the phenomenon in general. He says:[3] "C'est ici le cas de remarquer que lorsque deux grandes masses de roches acides se touchent, le plus souvent elles se trouvent réunies par une zone de passage plus ou moins puissante, dans laquelle les caractéres pétrographiques des deux roches sont, pour ainsi dire, confondus et mélangés.[4] In the same paper from which this quotation has been taken, the author cites many examples of such transition, among which that from granite to gneiss[5] and that from granite to "micro-granulite" may be especially mentioned. He explains them as due to an impregnation of the older rock by "les éléments fluides en voie de dégagement" from the igneous rock. When the temperature and pressure are suitable, a part

[1] Mém, de la Soc. Géol. de France, 2e sér. t. VI, p. 47.

[2] Abtheil. II, p. 272.

[3] Bull. de la Soc. Géol. de France, 3e sér., t. VII, 1878–9, p. 852.

[4] Cf. Ch. Vélain, Conférences de Petrographie. Paris, 1889, p. 6.

[5] Cf. LEHMANN, Untersuch. über die Ent. der alt. kryst. Schiefergesteine, p. 76. GREGORY, Q. J. Geol. Soc., 1894, p. 260 ff. BARROW has noted a complete amalgamation at the contact of granitite and diorite, of which the former is the intrusive member, Q. J. Geol. Soc., 1892, p. 121.

of the constituents of the older rock will become mobile and will tend to assume the same structure as those of the second period of consolidation in the younger rock.

McMahon pointed out how difficult it was to explain the presence of zones of transition about the Dalhousie granite of the Himalayas in some places and their absence in others on the old metamorphic theory of the central gneiss. He favored the opinion that, whether a zone of passage characterizes the contact or not, depends on the closeness of mineralogical similarity between the Dalhousie granite and the invaded strata. Only in places where the latter had been regionally metamorphosed did he find the appearance of gradual change from the granite into the country rock. The transition zone described by Lawson between the Laurentian gneiss and hornblende-schist in Rainy Lake region is so similar to the zones of the porphyritic granite that it will be well to read his own words on the subject: "Within the hornblende-schist, distinctly recognizable as such, there may occasionally be detected large crystals of red feldspar, which is quite foreign to these rocks, as if the feldspathic magmas had penetrated within the schist and crystallized there in the same large crystals in which they are wont to appear in the coarse gneiss."[1]

These authentic determinations of transgressive junctions between plainly eruptive rocks and their respective country rocks, coupled with the expectation that they should appear in contacts of that nature, lead us to follow Gregory [2] in concluding that they may form a useful criterion for a decision on an eruptive origin for massive rocks. So far as this principle is concerned then, the porphyritic granite can be eruptive.

The foregoing description of contact phenomena seems to us to indicate that the porphyritic granite has been intruded into the schistose rocks which have been grouped together in the terrane of the Lake Winnipiseogee gneiss by the survey. This conclusion is forced upon one as well in the study of the northern

[1] Ann. Rep. Geol. Surv. Canada, 1887–8, Part. F. p. 33.

[2] Q. J. Geol. Soc., 1894, p. 262.

extension of the "fishhook" as in the southern part to which attention has been so far called.

The porphyritic granite in contact with the Montalban group of schists.—We shall not consider in this place the grounds on which the survey has separated the Lake Winnipiseogee gneiss from the Montalban group. Our field observations have shown us pretty clearly that, although the lithological characters of the two terranes may be on the whole different, any distinction between them from a supposed difference of age is as yet without demonstration. Be this as it may, the problem before us can be solved without either proof or disproof of such a contention. At many points along the contact, the schists of the Montalban group are intersected by apophyses from the porphyritic granite, and appear as inclusions in the latter rock. The evidences of an intrusive origin for the porphyritic granite are of the same nature as in the case of the Lake Winnipiseogee gneiss, and are just as conclusive.

The Montalban group and porphyritic granite come in contact along the line running from Long Bay to Great Bay, and about one half mile east of Great Bay. Here a number of well-marked horses of the neighboring Montalban gneisses occur in the porphyritic granite. They possess a thoroughly gneissic structure, being as well foliated as their parent mass less than a hundred yards away. In this case the horses are not elongated and do not show any definite relation of position, either to one another or to the main contact line. Apophyses of porphyritic granite were also discovered along this part of the boundary.

An interesting occurrence of the gneiss appears in the road on the north side of Shaw's hill. It is isolated and completely surrounded by porphyritic granite whose nearest molar contact is nearly a mile away. This great horse is highly schistose, and several tongues of the granite here and there cut across the structure planes. The development of garnets in the horse may hint at some degree of contact metamorphism.

A second large horse which is some distance from its parent

terrane outcrops one mile and a half north of Holderness on the upper road. Here a gigantic slice of the country rock, about sixty feet wide and three hundred feet long, has been floated off and rests with its longer axis north and south, *i. e.*, parallel to the molar contact. It is composed of the usual biotite gneiss in which a thick sheet of hornblende-gneiss lies embedded, making up most of its mass. The latter has itself the appearance of being an eruptive rock which is thus older than the porphyritic granite. Associated with it are a large number of smaller biotite gneiss fragments which have no definite arrangement, but make a confused medley of discreet masses in the porphyritic rock. The whole looks like a huge flow breccia. Rather more than a half mile further north on the same road, there is a breccialike aggregation very similar to the last, even to its containing hornblende-gneiss folded up in a large mass of biotite-gneiss.

Three hundred yards west of where Dr. Dana's road leaves the four corners at New Hampton Centre we note another of those zones of transition between the porphyritic granite and the schists in contact with it. It is some twenty feet wide, and is strikingly similar in appearance to the case already described at Centre Harbor. Here the schist is cut by intrusive tongues with more or less sharp boundaries. These apophyses run across the gneissic planes.

On the extreme eastern end of the Sandwich Mountains at an elevation of about seven hundred feet above the Bear Camp River, a remarkable flow breccia or "permeation area"[1] outcrops in some extensive pasture fields. The rock presents all the features of a plutonic flow. The horses are here almost entirely hornblende-gneiss, some of them massive, both fine-grained and coarse-grained, others distinctly schistose. The porphyritic granite is on the whole granitic in appearance, but at the boundaries of the fragments, the feldspar phenocrysts are often oriented about them in a way which is strongly suggestive of a flow structure. The usual trendless nature of its constitu-

[1] Barrow, Q. J. Geol. Soc., 1893, p. 331.

ents is also lost in some of the tongues of porphyritic granite which penetrate the fragments in all directions. There the minerals are pulled out in planes parallel to the walls of the intrusion. The source of the hornblendic inclusions was discovered within a hundred yards of the breccia. An unknown thickness of the hornblende-gneiss lies interbedded in the biotite-gneiss. This great breccia outcrops at several places through a distance of three hundred yards along the base of the mountain and is only one hundred yards from the massive terrane of the Montalban group, which is continuous all the way from Morgan Mountain.

These occurrences of hornblende-gneiss in the porphyritic granite throw light upon the "hornblende rock" which was noted by Hitchcock in the Survey Report[1] as occurring to the east of Wickwas Pond. It covers altogether about an acre in extent. The rock is a hornblende-gneiss closely related in composition to the masses already described. It has a strong schistosity which lies parallel to the foliation of the granite enclosing it. The latter sends intrusive tongues into the gneiss which is evidently a large floe of the country rock moved far from its original source.

The small oval area of the porphyritic granite on the top of Mount Prospect is a stocklike body which suggests from its position an intrusive origin. Field study confirms this opinion. The rock is typical of the porphyritic granite in composition, in grain, and in the size of the phenocrysts. Within this porphyritic granite there are embedded several horses of the surrounding Montalban schists. The latter are extensively crumpled, perhaps by the intensity of the granitic intrusion. Again one can notice the parallelism of the phenocrysts to the margins.

The porphyritic granite in contact with the Rockingham mica-schist.—One of the most important localities in the state to suggest an intrusive origin for the porphyritic granite is on Saddle Hill, where that rock comes in contact with the Rockingham mica-schist. Here the formation is composed of well-

[1] Vol. II, p. 594.

foliated, fine-grained, muscovite-biotite-schist with abundant mica. The molar contact is found on the eastern end of the hill. It strikes N. 25° W., and is parallel to the schistosity of the mica-schist and to the pronounced foliation of the porphyritic granite. All the structure planes dip westward at a high angle. Going across the strike from the contact toward the porphyritic granite a remarkable series of elongated horses of the schist interrupt the continuity of the granite. They are usually much longer than their width, as, for example, a large one 150 feet long by 35 feet wide, which appears on the west side of the saddle. In most cases there is a definite orientation of the horses parallel to the contact line, while the foliation of the porphyritic granite wraps around the inclusion in a significant way. They are uniformly schistose with that structure as well developed as in the main body. Crumpling of the horses is also characteristic. For about two hundred yards east of the contact, the schist is cut by several intercalated sheets of porphyritic granite, varying from five to ten yards in thickness. Their phenocrystic feldspars lie parallel to the walls between which the sills were intruded. Similar sheets can be found in the pasture on the southern flank of the hill and west of Randlett Pond.

Relation of the porphyritic granite to the Waterville eruptives.— It is probable that the porphyritic granite is older than all of the intrusive rocks of the Waterville Mountains. The contacts were discovered in only one place, namely, on the southern slope of Mount Whiteface; there the hornblende-granite composing the mountain distinctly cuts the porphyritic granite. In the path from the Elliott House, at Waterville, to the top of the Sandwich Dome, many outcrops of several types of granitic rocks present a problem of correlation which the writer has had no opportunity to solve. It is possible that these rocks are chilled phases of the Conway granite; for there is, in the main, a tendency towards a porphyritic structure throughout, which on the one hand becomes more pronounced as one approaches the porphyritic granite, and is entirely lost in the very coarse Conway

(hornblende) granite in the bed of the Mad River. If this hypothesis be correct, the Conway granite is younger than the porphyritic granite, for at about 1800 feet above the river the porphyritic phase distinctly cuts the coarser rock.

The Ashuelot area.—The country rock about the porphyritic granite of the Ashuelot area is referred by the survey to three different formations. the Bethlehem gneiss, the schists of the Coös group and the Montalban group. Specific reference to this area was made in the second volume of the Survey Report.[3] In

[3] P. 470.

their general correlation, the survey considered the markedly oval form of this and other occurrences of the porphyritic granite as allying it in point of age to similarly shaped masses in the Archæan elsewhere. Such a form has a nearer homologue to the batholites described by Emerson in western Massachusetts, and as we shall see, these similar forms have similar origins. That the porphyritic granite of the Ashuelot area is eruptive and of an intrusive nature can be amply proved. We shall not attempt to trace the evidence from the contact-phenomena, as it might be traced in a complete description of the whole boundary. It is of the same nature as that outlined for the Winnipiseogee area. With that fact in mind, we have considered it expedient to refer to a few only of the possible localities which can be readily visited for confirmation of our views.

A representative contact of the porphyritic granite and Coös mica-schist outcrops where the boundary line between them crosses the road running southwest from Ashuelot over Gun Mountain. Here and along the western ridge of Gun Mountain the typical biotite-muscovite-schist is strongly charged with interbedded actinolite-schist and quartzite. Several apophyses of the porphyritic granite cut the schist. One of them, twenty feet wide, cuts across the strike, extending a considerable distance from the contact before it disappears under the soil-cap; another over two feet in width is also well exposed, but lies nearly in the planes of schistosity. Horses of schist are embed-

ded in the granite, but the overlying loose deposits prevent the discovery of any definite arrangement among them. The granite itself has a good parallel structure, and it is important to note that its structure-planes are conformable in strike and dip with those of the adjacent schists. This same conformity was several times observed along this western side of the area. Sometimes, even at the contact, the porphyritic granite is quite granitic without a trace of the foliated structure. A good example of this appears at the contact on Hall's hill, near Chesterfield factory. At this locality, too, there is no doubt as to the relation of the two formations. The intrusive tongues of porphyritic granite cut across the schists and associated gneissic bands in a very marked way; the sharp contrast of grain and composition enabling one easily to differentiate the igneous masses. Occasionally dikes of the porphyritic granite may be found traversing the schists at a distance from the contact. At the three corners, about a mile south of Chesterfield, numerous great veins of coarse pegmatite outcrop and with them occurs a set of true porphyritic granite dikes which are probably apophyses of the main mass, half a mile away.

Facts of like character refer to the contact with the Bethlehem gneiss on the eastern side of the oval. Field evidence shows that the latter is of the same metamorphic epoch to which the Coös mica-schist is referred; thus, the argument regarding the better exposed part of the boundary applies to it in its entirety.

Fitzwilliam area.—We have seen already that the survey suspected an eruptive origin for certain parts of the porphyritic granite, and had cited facts from the Fitzwilliam area as in part the basis for the conception. The contact line of this little patch of the rock is very clear in its teaching. Even more graphic than that of the bowlders described in the survey report[1] is the evidence where the rock is in place. About a mile southwest of the village the porphyritic granite is found in a pasture field by the roadside. Included in it are many horses of biotite-

[1] Vol. II, p. 471.

gneiss, some of which are large, being as much as twenty-five feet in diameter. Within one of the latter an interesting irregular injection of the porphyritic granite is well exposed. One feature exceptionally well shown is the fine-graining along the margins. At this place, too, dikes of the Concord granite cut the porphyritic granite, and the former rock is thus the youngest terrane in the region. Since it surrounds the porphyritic granite on all sides, this Fitzwilliam occurrence may itself be a large floe brought up from below from a much larger mass.

The Main area.—Our observations on the Main area were extended only to its southern half. The great thickness of the various glacial deposits make it, on the whole, less satisfactory for a study of contact relations than the Winnipiseogee area. We have aimed in the course of a somewhat hasty examination to discuss the facts as regards the schists grouped by the survey under the names of the "Ferruginous slates," and the "Ferruginous schists." In both, the rocks consist of two-mica-schists, biotite-schist, biotite-gneiss, and muscovite-biotite-gneiss, all of which may be garnetiferous. Between them we can trace no definite distinction, either of composition or of age, and the area on the eastern border, marked "Lake Winnipiseogee gneiss," encloses stripes of schistose rocks which are identical with those of the above-mentioned groups. Here, as often elsewhere, the grounds for the subdivision carried out by the survey do not appear in the field.

The great sill of porphyritic granite to the north and northeast of Greenfield has one contact well exposed with interruptions for the distance of a mile. It is an intrusive one. The granite was erupted into the schists along a plane of foliation, and there is the usual development of parallel structure in many of its outcrops which accords with that of the walls. The intrusion is, on the whole, sheet-like in its form, though in some places where the schists are intensely folded, it cuts across their structure-planes. In such cases the foliation of the porphyritic granite remains parallel to the boundary line. The apophyses penetrate the schists irregularly in all directions, generally pay-

ing no attention to planes of weakness. They are finer-grained than their parent sill, but show here and there a feldspar as much as two inches in length.

One of the most interesting parts of the contacts is that which belongs to the area marked "Lake Winnipiseogee gneiss" by the survey in the towns of Antrim and Hillsboro. It is exceptionally well exposed at intervals for a distance of five miles, and especially on the long ridge some two miles west of Antrim. The name thus given the rock with which the porphyritic granite here comes in contact is a decided misnomer. The new terrane consists of an ancient metamorphosed eruptive cutting the "Ferruginous" rocks in every way similar to the schists that extend from Bennington to Henniker. This complex is itself cut by the porphyritic granite.

The older eruptive rock is a typical coarse granitite containing a good deal of muscovite which is all secondary. The quartz is the common blue variety of New Hampshire crystallines. Both orthoclase and a basic plagioclase occur, but they are generally badly decomposed. It is a difficult rock to diagnose thoroughly on account of the vast amount of crushing which appears in the thin section. The quartz and feldspars are much granulated marginally, the unbroken cores showing the characteristic wavy extinction. The plagioclase lamellæ are often bent through large angles. Minute faulting is common in them, and throughout the slides the shreds of biotite are bent and twisted in a striking manner, while the extinction on the base of biotite plates is most irregular. In fact the condition of this rock is in marked contrast with that of the porphyritic granite close by. The granite is quite without any signs of serious disturbance; the granitite has endured the very severe mechanical strain of extensive mountain-building. Often the signs of developed schistosity in this once massive rock are easy to discern in the ledges, and these new structure planes strike a few degrees east of north, *i. e.*, they lie in the main parallel to the strike of the "Ferruginous" terrane.

Now, within the crushed granitite there is an extraordinary

display of inclusions, varying in size from small fragments to masses twenty feet square, all of which have evidently been derived from the older schists to which they are mineralogically and structurally similar. These horses are highly ferruginous, and weather with the same rusty appearance that characterizes the parent rock. So great is their number in some places that considerable stretches are veritable flow breccias. But it is rather the remarkable crumpling and other evidences of intense folding which attract one's attention to these outcrops. The sliverlike horses are very often bent into sigmoid flexures; sometimes one is seen to be completely doubled back on itself in a nearly closed fold. They are usually much jointed, and here and there actual movement along a fault plane may throw one part of the inclusion a foot or more out of its normal continuity. While there is not much difference mineralogically between these inclusions and the rock of the ferruginous terrane, yet there is some evidence of a metamorphic change due to the granitite. About one of them, some two feet long and a foot and a half broad, in particular, a two-inch zone filled with large biotites was developed. The biotite of the granitite itself is often segregated in large individuals. Many of the horses have been considerably melted up, and it is probably the absorption in this way of so much of the iron oxides that conditions the characteristic deep reddish brown color of the weathered granitite.

This terrane has but few affinities with the simple Lake Winnipiseogee mica-gneisses, where they occur in their normal fresh uncrushed habit. No evidence is yet forthcoming that the latter are eruptive. Not only do these multitudinous horses prove the eruptive origin for the granitite, but its actual contact with the ferruginous schists was found on Riley Mountain, and it tells the same story. The usual field criteria of the presence of horses, apophyses, and intrusive sheets are there exhibited. A rock zone of the foregoing description, averaging rather more than a quarter of a mile in width, runs southward five miles to the Antrim ridge above mentioned, always appearing between the porphyritic granite and the main body of schists.

At many places along its western margin the porphyritic granite cuts the granitite, but often passing into it by a zone of transition analogous to those described in the Winnipiseogee area. Thus we have added another to the crystalline terranes which have been profoundly affected by dynamic processes since their formation; they are in this respect to be contrasted with the younger relatively unaltered porphyritic granite, and lastly, they are of interest not only from their relation to the history of igneous activity in the state, but also from the light that they throw on the age and origin of the granite.

REGINALD ALDWORTH DALY.

(To be continued.)

THE MEASUREMENT OF FAULTS.

ACCORDING to the definition given by Dana, "faults are displacements along fractures." Whenever the rocks of the earth's crust are subjected to strain, fractures take place in them as in any other body under similar conditions, and the different parts of the rock tend to move past one another along the fracture-planes, seeking to obtain relief from the strain and to accommodate themselves to new conditions. In this movement one part of the fractured rock-mass may move upon the other in any direction, up, down, sidewise or obliquely, according to the conditions, which are different in each instance. There is, so far as I know, no law governing the direction of movement in faults which is of any use in geological diagnosis. Naturally, when there is any preëxisting plane of weakness of the rock which is subjected to strain the movement takes place by preference along this plane; and, hence, in sedimentary beds, it is probable that movements along the stratification planes constitute the commonest variety of faults. Inasmuch, however, as the beds in disturbed districts lie in every conceivable position, the probability just stated does not give any clew to the average attitude of faults.

The movement in faults can be completely ascertained only by the aid of independent and accidental phenomena. In homogeneous rock-masses (leaving out of consideration fault scarps, fault gulches, and other topographic phenomena, and treating the faulted mass as a solid without boundaries), the amount of movement cannot be ascertained or even approximately estimated; although the *existence* of a fault can be determined by the records left on the slipping surface or surfaces in the shape of ground-up rock or fault-breccia, in polished and striated rock faces, and so on. It is certain, however, that the amount of friction as displayed by trituration and polishing is not neces-

sarily proportionate to the amount of movement, since faults with slight displacement are often accompanied by zones showing profound trituration, while others of far greater movement show to a much less degree the effects of friction. The friction in each case seems to depend upon the angle of the chief stress to the sliding plane, rather than on the amount of movement along this plane. In heterogeneous rocks the amount of movement of a fault can ordinarily be estimated with more or less accuracy, the degree of closeness depending upon the nature of difference in the composition of the rock-mass. In such heterogeneous rocks the amount and direction of a fault movement must be judged by any available phenomenon or phenomena. By far the commonest variations in rock-masses which are constant enough to be reliable as data are sedimentary beds, and therefore the commonest means of measuring a fault movement is the separation of the two parts of an originally continuous stratum. On this account it is easy to fall into the error of considering faults simply as dislocations of strata. In careful geological work, however, such as mining work must necessarily be, it is important to cultivate a more correct conception, and to regard sedimentary beds as phenomena accidentally associated with faulting, whose dislocation must be associated with all other available criteria, each one as valuable as the other, to determine the amount and direction of the total movement or displacement. Any fault, for example, in which the direction of movement is parallel with the plane of sedimentation will not cause any apparent displacement in a sedimentary bed; and this may be the case in faults having any conceivable attitude, since the sedimentary beds themselves may be folded so as to stand in any conceivable attitude with reference to any fixed plane, such as the earth's surface.

When the direction of movement in a fault lies at a slight angle to the plane of sedimentation, the apparent displacement of a stratum resulting from this fault will be only a slight part of the actual fault movement; and it is only when the direction of movement is perpendicular to the plane of sedimentation that

the separation of the parts of the faulted stratum is an accurate measurement of the movement. Theoretically speaking, the chances are infinitely against any such coincidence, and in actual practice it is rare that the movement may be even approximately estimated in this way. In mining geology it has been found that the most valuable criteria for measuring faults are, besides sedimentary beds, igneous bodies, such as dikes ; bodies of ore ; striæ on the fault plane, showing the direction of movement ; and the composition of the fault breccia, which may show, in some degree, the amount of movement. By taking several of these criteria together it is often possible to actually ascertain the movement of a fault.

It is sometimes possible to find out the amount and direction of movement immediately ; but more often it must be indirectly calculated, and to do this it is important to have clearly in mind the nature and value of some of the principal functions of a fault movement, and to have specific terms by which to designate them. The terms already in use are of a rather vague and general character, resulting from the usual conception of a fault as a dislocation of strata ; the four generally employed are *displacement*, *throw*, *heave*, and *offset*. The words *displacement* and *throw* are used interchangeably, and commonly refer to the separation of beds by a fault as seen in a vertical section. Each of these terms is used by some to indicate the distance along the fault plane between the broken ends of the bed as seen in the section, and sometimes the perpendicular distance between the parts of such beds, projected, if necessary. There is no agreement, however, which definitely assigns the terms to separate measurements, and, indeed, it is very common for a writer to use the terms interchangeably for one or the other function. *Heave* and *offset* are also used interchangeably, and are usually held to signify the perpendicular distance measured on a horizontal plane, such as the earth's surface, between portions, projected, if necessary, of a bed separated by a fault.

In mining work it is generally necessary to clearly differentiate the different functions of a fault movement, and I have

adopted the following terms descriptive of the most important of these; these terms include nothing very novel in the way of nomenclature, but are intended simply to affix definite names to definite things.

Dislocation and *displacement* are general terms, applicable to any part or the whole of a fault movement. Each of the functions defined below, and to which specific names are given, may be called simply a dislocation or displacement.

Total displacement is the distance which two points originally adjacent are separated by the fault movement; the line connecting these two points lies in the fault plane in all straight faults. It is occasionally possible to determine the total displacement directly by such criteria as the separation of the parts of an ore body, the intersection of a given dike with a given stratum when found on both sides of the fault, and in other ways; but ordinarily it can only be calculated or approximately estimated from some of its more easily measured functions.

The *lateral separation* is the perpendicular or shortest distance between the two parts of any continuous zonal body (such as a sedimentary bed), which has been separated by a fault, the distance being measured along the fault plane. The lateral separation may be measured in a vertical, horizontal, or oblique line, according to the attitude of the bodies between which it is measured, and in any fault it may vary from zero to the total displacement. In the case of dikes cutting sedimentary beds, of marked unconformity, of abrupt folds, and so on, it may be possible to measure two or more lateral separations in a single fault. In this case, and in a number of others which are possible, the total displacement may often be calculated from the lateral separation, since the latter is always the side of a right triangle of which the former is the hypotenuse.

The *perpendicular separation* is the perpendicular distance between corresponding planes in the two parts of any single body available as criterion (such as a sedimentary bed), when this body has been separated by a fault, the planes on each side of the fault being projected for the purpose of measuring, if neces-

sary. The perpendicular separation thus has a certain relation to the lateral separation; for it constitutes a side of a right triangle, the hypotenuse of which is the lateral separation, except in the possible case where the perpendicular and lateral separations coincide.

This mathematical relation makes it often possible to estimate the lateral separation from the perpendicular separation, and from the latter the total displacement. Of these three functions, the perpendicular separation is most easy of measurement, and its value may vary from zero to the full amount of lateral separation. The lateral separation is easier to ascertain than the total displacement, and its value may vary from zero to the total displacement.

The measurements which have been defined have no constant direction, since they refer to fault movements which are capable of infinite variation. In general geological work, however, it is often only possible to measure fault movements along certain arbitrary planes. The most valuable of these planes, are the earth's surface, which may be considered a horizontal plane, and vertical sections, into which available data are put, with the gaps in the chain of information often theoretically filled out. In such cases, where some dislocation is evident, but the information is so meager that it is not possible to know the fault so accurately as to estimate even approximately its total displacement or lateral or perpendicular separation, it is necessary to employ specific terms to designate the known or estimated dislocations, although the relations of these dislocations to the total displacement may be unknown. For this purpose the terms *offset*, *throw* and *vertical separation* may be used. The terms *throw* and *vertical separation* are applied to the dislocations of a fault as seen in a vertical section; the term *offset* to the dislocation as seen in a horizontal section, such as the earth's surface may be considered to be.

A *throw* may be defined as the distance between the two parts of any body available as a criterion (such as a sedimentary bed), when these parts have been separated by a fault, the dis-

tance being measured along the fault plane as shown in a vertical section.

The *vertical separation* is the perpendicular distance between the intersection of the two parts of any faulted body available as a criterion (such as a sedimentary bed), with the plane of a vertical section, the lines of intersection being projected if necessary for the purpose of measurement. In perpendicular faults the vertical separation is identical with the throw; in all others it is less than the throw, but sustains a certain relationship to it, being one side of a right triangle of which the throw is the hypotenuse. Thus the vertical separation may vary from zero to the full amount of the throw. The throw is always a part of the total displacement, although with no definite relationship to it, and varies from zero to the full total displacement.

The term *offset* may be used to designate the perpendicular distance between the intersections of corresponding plane in the two parts of any faulted body available as a criterion, such as a sedimentary bed, with a horizontal planes such as the earth's surface may be considered to be; the planes being projected for the purpose of measuring, if necessary. Like the throw, the heave or offset is a part of the total displacement, but has no definite relationship to it.

To sum up, there are six terms proposed to designate the different parts of a fault movement, each term applying to a measurement which varies in accuracy and proximity to the total displacement in proportion to the available amount of information. For general outline work where accurate data are not obtainable, the terms *throw* and *vertical separation*, referring to the measurements of a fault at its intersection with a vertical plane, and the term *offset*, indicating a measurement of a fault at its intersection with a horizontal plane, are adopted. The throw and offset are parts of the actual fault movement, but of unknown value, while the vertical displacement sustains a certain relationship to the throw. Where more complete data are obtainable, the terms *total displacement*, *lateral separation*, and *perpendicular separation* are adopted. The perpendicular separation sustains a certain rela-

tionship to the lateral separation, as the lateral separation does to the total displacement.

The terms which have been adopted above have purposely been made as few as is consistent with the plan of furnishing a scheme for complete fault-analysis. The number might be increased indefinitely; yet ordinarily this is undesirable, for most other fault measurements are simple mathematical functions of the terms above adopted, and can be easily reduced to one of these; and the great multiplication of terms leads to confusion in a study which is at best not too simple. In specific instances, however, it may be desirable to increase the number of terms, and to give separate names to other fault measurements.

J. Edward Spurr.

THE DRIFT AND GEOLOGIC TIME.

ASSUMING the correctness of the ice-sheet or glacier theory of the origin of the drift which, according to one supposed to be of high geological authority,[1] "has passed from the region of hypothesis to that of demonstration, and should form the basis of all reasoning on the subject," there are still many problems that are open to discussion and in regard to which writers widely vary in their opinions and statements. One of these is the lapse of time between the beginning and the end of the period, that portion of geologic time required to prepare for and lay down the drift deposits and to return to present climatic and physical conditions. Of late years there has been a tendency in some quarters to reduce the estimated length of this period. According to Prestwich[2] 25,000 to 35,000 years would suffice for the whole period of formation and retreat of the ice-sheet. Professor Wright endorses this estimate,[3] and Warren Upham[4] even abbreviates it a little, allowing only 20,000 or 30,000 years for the actual glacial period, and 6000 to 10,000 years for the since intervening time. Becker,[5] figuring on astronomical data, thinks that conditions favoring the glaciation of the territory covered by the drift may have existed within 40,000 years, and presumably gives that as the probable outside figure for their occurrence.

On the other hand, perhaps the larger number of glacialists have allowed in their estimates very much longer periods. To say nothing of Croll, Ramsay, Geikie, and earlier writers, we

[1] Geol. Mag., new ser., Decade IV, Vol. IV, 75, February 1897. (Review of Croll's Life and Work.")

[2] "Geology," p. 534, 1888.

[3] Man and the Glacial Period, p. 364, 1895.

[4] Bull. Am. Geol. Soc., V, 99, 1894.

[5] Am. Jour. Sci., 3d ser., XLVII, 95–113.

have, amongst recent writers, R. Bell,[1] who thinks the estimates of the above-mentioned authors *not* excessive. Chamberlin[2] estimates the lapse of time since the Kansan epoch to be equal to fifteen times the lapse since the last epoch, which would give a high figure for the whole period on any tenable estimate of this last factor, and to this is to be added an undetermined number of years for the pre-Aftonian (Albertan) stage. Penck, at the recent Toronto meeting of the British Association, is reported to have allowed at least 500,000 years for the glacial epoch, including all the interglacial stages, and very recently F. B. Taylor,[3] in an article on the moraines of recession of the latest (Wisconsin) ice-sheet, gives as his estimate of the time required for the retreat of this single ice invasion from the latitude of Cincinnati to the straits of Mackinac, a period of from 75,000 to 150,000 years. Adding to this an equal lapse of time for its advance, and we have an estimate of 150,000 to 300,000 years for the whole time occupied by this most recent member of the drift.

In addition to the actual periods of the ice occupancy of the territory we have to reckon in the interglacial epochs of which there is considerable evidence, and which must materially add to the length of Pleistocene time. I have not seen many estimates of the time required by these, the most noteworthy one being that of Professor McGee, of the time required for the deposition of the forest bed overlying the earlier Iowa till. Taking for his unit the period of written history, the very least figure he gives for this formation is about 112,000 years, and this is far exceeded by his estimate of its possible maximum duration. The thickness of some of these intercalated beds would naturally indicate a considerable period for their deposition, but McGee's estimate certainly seems an extreme one, and is the more noticeable when considered in connection with the relatively very short allowance of time he has given for the ice invasions themselves, even allowing, as we should, that he is

[1] Bull. Am. Geol. Soc., I, 295.

[2] Jour. Geol., IV, 875, 1896.

[3] Jour. Geol., V, 1897, July–August.

considering only the peripheral portion of the ice-sheet and not its greatest development. The length of the interglacial epoch must in any case enter as a very important element in our estimates of the total time required for the deposition of the drift, and the limited data from exposures are at best more suggestive than definite in the information they convey as to this point.

With these divergent views as to geologic time it would not appear as if the glacier theory afforded a very satisfactory basis for reasoning upon this particular phase of the subject. There certainly seem to be decided difficulties in utilizing the drift phenomena for the measurement of geologic time, each apparently possible solution of the difficulty seeming to present still more impossible problems. Some of the estimates made appear to be compromises, therefore, or alternatives, accepted only as better than something else. Thus Prestwich and Wright find it easier to limit the duration of the glacial period than to admit the possibility of man having existed 80,000 years on the earth, or that the fauna or flora of today could possibly be the same as that of 240,000 years ago. The elements of individual prepossession and mental idiosyncrasy enter largely into the consideration of scientific questions, and all the more into such as this where the chances of legitimate difference of opinion are so ample.

It is the object of this paper to call attention to the method of calculating geologic time by the transportation of erratics, a method that has up to the present time hardly received the attention it deserves in the literature of the subject. It is at first sight a little remarkable that this should be the case. That some at least of those who have alluded to it appear to have discredited its value is also remarkable, as any method that adds any degree of certainty to our estimates ought to be regarded as a boon to science. While, as I shall attempt to demonstrate, it has its value even with the rather indefinite notions we have hitherto had as to the flow of continental glaciers, recent researches by Chamberlin and others on the Greenland ice-sheet have added very much to its importance and applicability. We

have by it at the present time the data which enable us to form a definite minimum estimate of the time required for the deposition of the drift in North America, on the presumption that this was done through the agency of land-ice, or glaciers. If it is assumed that the drift was water laid, either altogether or to any considerable extent, altogether different elements enter into and affect the calculation, but that is not the assumption of the present paper.

It is perhaps conceivable that the climatic conditions during the formation of the ice-sheet were such that it was deposited by precipitation simultaneously over the greater part of the area it occupied. The névé, in other words, might have been almost coterminous with the glacier, only a narrow external rim being excluded. The evidence, however, of motion throughout at least the greater part of its extent is afforded by the erratics many of which have traveled 600 or 700 miles or even more from their original beds. A bit of jasper conglomerate found south of Cincinnati must clearly have traveled from the north shore of Lake Huron, and fragments of Archæan or eruptive rocks found abundantly along the southern limit of the drift in Illinois could have had no nearer source than northern Wisconsin 500 or 600 miles away, if indeed they have not a still more northern origin. A bowlder or pebble from the north shore of Lake Superior, if found in southern Illinois, would have traveled nearly or quite 800 miles, and while I am not sure that any such have been identified, their occurrence there is altogether within the bounds of probability. Such erratics, according to the glacier theory, must have been conveyed as subglacial or intraglacial detritus and must have progressed with the ice certainly at no greater rate than the ice itself and almost certainly at a much slower one. We have no certain evidence what the progress of the glacier was, but it could not have been a rapid one. In existing glaciers the most rapid rate of motion is about seventy-five feet a day, but this occurs only during one or two months in summer, and in two or three exceptional Greenland glaciers where the ice, so to speak, is under pressure down a favorably

inclined valley from the great Greenland ice-cap, the nearest analogue to the immense glaciers of the drift period with which we have at present any satisfactory acquaintance. These rapid flowing glaciers are exceptional in Greenland, where the general movement of the ice is unquestionably very slow. They can be compared to rapids at the outlet of a lake. The Greenland ice-cap far overtops the bordering mountains, and yet in only some seventeen places along the whole Danish Greenland coast are there free outflows to the sea. While we know less of the other portions of the coast the general character is the same; a rapid motion is exceptional and it is a reasonable certainty—to quote Taylor[1] whose paper contains the latest discussion on this ice motion —"that the average movement of that portion of the border of the Greenland ice-cap that rests upon the land is extremely small. Of that portion which ends in the sea only a small fraction has a high rate of motion, as is shown by the lack of activity in the discharge of icebergs. When it is considered that the land border is very much greater than the sea border, and that of the sea border a portion has a relatively slow movement, it will be evident that the average rate of movement of the great ice-sheet of Greenland cannot be high; and the average rate of this border is the nearest available analogue to the border movement of the still more extended periphery of the ancient American or Laurentide glacier."

In fact it is impossible, when we consider that the Greenland ice-cap only abuts on the sea along a small portion of its border in the form of glacial tongues, and that the average movement of these is so small, not to believe that towards its interior the ice movement must be almost imperceptible—almost if not absolute stagnation. The Antarctic ice-cap is very little known to us, but its movement must also be very slow, judging from the discharge of icebergs. All the icebergs of the North Atlantic come practically from a few Greenland glaciers, making up altogether only a minute fraction of the whole Greenland coast. In the Antarctic, on the other hand, we know of hundreds of

[1] Jour. Geol. Vol. V, 442, 1897.

miles of continuous ice cliffs ending directly in water hundreds of fathoms deep, and have reason to believe that this is only a fraction of what exists, and yet icebergs are sometimes almost unknown in the southern seas for years at a time. Again there will be years in which they are abundant and extraordinary in size, but at no time is their quantity comparable to what ought to exist were the discharge anything like a free one along the barrier.[1] It can be reasonably assumed, therefore, from what we know at present that the movement of the southern ice-cap is also extremely slow, notwithstanding the favoring conditions of direct discharge into deep water.

The great Laurentide glacier, extending over four million square miles of surface, can also be safely assumed to have had a very slow motion as a whole, fully as slow as that of the Greenland or southern ice-cap. In fact the question arises, and is not at first sight readily answerable, how it had any motion at all. Gravitation certainly had less play than in Greenland, for instead of an area nowhere more than three hundred miles from the ocean [1] we have one eighteen hundred or two thousand miles in

[1] "As has already been stated, there are years of very few or no icebergs, and then years when great numbers are reported. In the year 1832, the southern ocean was so covered with icebergs that a number of whaling vessels, bound round Cape Horn, encountering them, put back to Valparaiso to await a more favorable season, because it appeared too dangerous to undertake the voyage. Again in 1854 there was a great accumulation of icebergs, and now during the past few years, notably 1892 and 1893, there has been another notable output from the great berg factories of the Antarctic regions. During the intervals between these periods there have been very few bergs reported. What causes this occasional great accession of bergs? Some authorities offer as a probable explanation the breaking off of the ice margin by volcanic eruptions, and others that earthquakes cause numerous pieces of the glacier to become detached and set adrift as icebergs, and others that unusual heavy annual snowfall is favorable for increase in number of bergs. The rapidity of glacier movement seems usually to regulate the number of bergs cast off. If the ice at the bottom of the glacier moves so slow that the melting of the margin on coming in contact with the salt water equals the advance, then we would have no icebergs, except perhaps those breaking off from the upper part of the outer margin, and these would be comparatively small." W. T. Gray, M. S. U. S. Hydrographic Office, Pilot Chart, N. Pacific Ocean, Nov. 1899.

[1] Prof. Chamberlin finds no evidence that the ice-sheet of Greenland ever very greatly exceeded its present limits.

diameter and one in which no reasonably supposable elevation could give a uniform slope varying appreciably from the horizontal. An elevation at the center of radiation of ten thousand feet (which is much beyond the most favorable interpretation which any known data will bear) with an ice-cap of as much more, would only make a slope of under half a degree in eight hundred miles, and of considerably less in some directions to the outer limits of the ice. Some glacialists, however, are liberal in their allowances of earth movement to account for the flow of the glacier. Mr. Upham,[1] for example, thinks that the strong current needed to transport bowlders from the southeast shore of Hudson Bay one thousand miles southwestward to southern Minnesota, would require a slope of at least fifty feet or more per mile, apparently unmindful of the fact that such a slope for the given distance would require an elevation of the ice-cap to the height of 50,000 feet, where precipitation would, if it occurred at all, probably be so slight as to seriously embarrass the formation of any considerable ice-cap whatever. It is not probable, however, that there was any uniform slope over the glacial field, and whatever effect was produced by gravitation could not be such as would cause a rapid motion of the ice, "faster than the Swiss glaciers."[2] The other theories that have been invoked for the glacier motion, the effects of thawing and freezing, expansion under varying temperatures, etc., are none of them, we think, counted as sufficient to cause rapid movement in so large a mass, as a whole, even by their upholders, and such estimates as two to five feet per day are hardly based upon a due consideration of the probable or possible physical conditions. Dana's[3] estimate that "the rate of motion could hardly have exceeded a foot a day, and may have been in most parts no more than a foot a week" is much more likely to be near the truth. Ice, except under special conditions of pressure or *vis a tergo*, barely moves on a slope of one degree, and an average slope

[1] Greenland Ice Fields and Life in the North Atlantic, p. 304.

[2] WARREN UPHAM. Bull. Geol. Soc. Am., III, 401, 1892.

[3] Geology, 3d ed., p. 539.

of a quarter of a degree for a thousand miles would require an elevation at the point of origin of the flow of something approaching five miles. The probability, and we may say the certainty, is that the slope was not uniform and that over large distances the ice traveled over dead levels, and in parts even stagnated, the upper part flowing over the lower débris-laden portions. That the contained débris has a retarding influence on the flow of glaciers has been urged by O. P. Hay,[1] I. C. Russell,[2] and R. D. Salisbury,[3] and it appears that this may even cause absolute stagnation under some circumstances. Even detached erratics seem to progress more slowly than the body of the ice in certain instances; witness the well-known observation of Professor W. H. Niles[4] on the Aletsch glacier where ice moved so much more rapidly than a contained bowlder as to leave a free tunnel for a considerable distance on its lee side. Even lighter substances appear to be occasionally retarded in their progress as compared with the ice. Recognizable remains of buried travelers have been taken out of Alpine glaciers even hundreds of years after their loss.[5] It is impossible, therefore, to claim that these erratics could have traveled at the same rate as the surface of the glacier, and when we consider that they bear the marks of having been subjected to scouring in the ground moraine that has left their surfaces flattened and striated, the probability of such a rate of progression is certainly very much diminished.

Allowing a flow of two feet per day to the ice-sheet, which is undoubtedly far above the real rate of the ice movement, it would require 7200 years for Mr. Upham's bowlder to travel its thousand miles from Hudson Bay to southern Minnesota, and this without any delay from friction or attrition in the ground moraine, or stagnation in the lower strata of the ice. Taking, also, into account the fact that the northern erratics are found at all dis-

[1] Am. Jour. Sci. and Arts, 3d ser., XXXIV, 52, 1887.

[2] Jour. Geol., III, 823–883, 1895.

[3] *Ibid.*, Vol. IV, 769–810, 1896.

[4] Am. Jour. Sci., 3d ser., XVI, 367, 1878.

[5] Sir Henry Howorth. Geol. Mag. N. S., Decade IV, Vol. IV, 127.

tances from their point of origin and at all levels in the drift, it seems sufficiently clear that we cannot measure the duration of any single ice invasion by the period required for the transportation of a single erratic from its northern origin to its outer verge, even allowing for all the retardation in the ground moraine. Every one of the Archæan fragments so commonly seen along the southern borders or the drift must have required some four thousand years, even if we allow it to have advanced two feet a day, to reach its present position and probably a much longer period, for there is no good reason to suppose that the mass of the ice-sheet itself advanced at any such rate. We can also allow a somewhat more rapid transportation of débris near the margins by floods, subglacial drainage, etc., and yet find our time limit tending to be too small. The Alpine and Scandinavian glaciers, with their steep gradients affording full play to the action of gravity, move on an average only a few inches a day. How the continental glacier derived its movement, except at its elevated origin and near its periphery, is one of the questions that no one has yet satisfactorily answered, and the inevitable conclusion from the known facts is that while motion undoubtedly occurred it must have been extremely slow.

The formation of the ground moraine must have required a very prolonged period of time, involving as it did the grinding up and working over of the rocks and other material that together make up the till. It does not matter whether it is held that it was mainly deposited under the ice-sheet by stagnation of contained débris, as suggested by O. P. Hay,[1] or as a continuous terminal moraine as the glacier retreated, as was held by Newberry.[2] In either case a long time must have been required.

In what has been said I have tried to show that a great ice-sheet thousands of feet in thickness, extending over a third of a continent, expanding from its center in the direction of least resistance towards its periphery, having over the greater portion of its area a very slight slope, and probably none at all

[1] Loc. cit.

[2] Geol. Survey of Ohio, Vol. II, 29; III, 34.

in parts; hampered by inequalities of the underlying surface and by the detritus it shears off from these, must have had a very slow, though irresistible, progress; and that, accepting the existence of such an ice-sheet and taking account of this slow rate of progression, the contained erratics, whose origin can be identified by the situations in which they are found and the distances they have traveled, will afford a better means of making an approximate minimum estimate of the duration of the Pleistocene period than any other at our command. By this we can assure ourselves with almost absolute certainty that a single ice invasion could not have taken place carrying a single erratic from north of the lakes to the southern limit of the drift in less than four or five thousand years, and this without taking any account of the time required for the change of climate, the gradual gathering of the ice, its recession, the probable slower motion of the erratics than of the ice mass as a whole, or its retardation by friction, as evidenced by its facetted and striated surfaces. Taking all these into the reckoning, we ought, it would seem, to triple or quadruple the time; and if, instead of taking the highest estimates of glacier motion, we accept the more reasonable and probable ones, the period will be still more prolonged. It is difficult to see how, under these circumstances, a single ice invasion could have begun and run its course within the limits of less than thirty or forty thousand years; and if we accept Dana's estimate of the glacier flow at one foot a day for a maximum, and one foot a week as a possibility, we would have to carry our figures very much higher. We have, however, according to some of the highest authorities, Chamberlin, Leverett, and others, five separate ice invasions to account for, besides interglacial periods of possibly equal or greater duration; and this greatly magnifies the necessary estimate of time for the whole glacial period. One of these ice invasions appears to have transported bowlders one thousand miles, which, at the liberal rate of two feet per day, would require 7200 years, and the others, from the average extreme distance to which erratics were transported, will equal certainly 500 miles, which would require

3600 years, or, in the aggregate, 14,400 years. The total, therefore would be, in round numbers, about 22,000 years for the mere transportation of a single erratic in each invasion, and that at an improbable rate of speed and without any allowance whatever for the time occupied in the formation, culmination, or retreat of the glacier, or for interglacial periods. It is sufficiently evident from these figures that the ice-sheet theory of the till formation is utterly incompatible with such estimates as those of Prestwich and Wright, which give the whole glacial period a duration of only 20,000 to 40,000 years.

It has been matter of surprise to me that so little weight has been given by authorities to the arguments from the calculation of the transportation of erratics in the estimation of the duration of the glacial period. Most of them absolutely ignore, or at least fail to utilize it, and those who do allude to it at all, like Helland,[1] give it only the briefest and most casual mention. It appears to me to be the one method by which we can obtain, not the actual, but the utmost possible minimum of duration of such an ice-sheet as the generally accepted glacial theory demands.

Professor W. J. Crosby[2] has offered the suggestion that, as the great mass of the rock débris of the till is local and has never traveled far from its place of origin, the northern erratics were transported largely by water in the glacial lakes that formed along the borders of the ice-sheet. Inasmuch as these are found throughout the till at all levels, his suggestion amounts practically to an admission that the whole mass in which they are disseminated was thus deposited, which is altogether inconsistent with the general tenor of his argument, and is almost, if not quite, equivalent to giving up the land-ice theory of the deposition of the till.

It may be worth while here to notice one or two estimates or statements in regard to the duration of certain stages of the glacial period by prominent glacialists. The recent estimate of

[1] Zeitschr. der deutschen Geologischen Gesellsch. XXXI, p. 76, 1879.

[2] Am. Geologist, XVII, 1896, p. 234.

F. B. Taylor of from 75,000 to 150,000 years for the recession of the Wisconsin ice-sheet from Cincinnati to Mackinac has been already alluded to in the early part of this paper. As against this apparently large, but possibly not too large estimate (that is, admitting the land-ice formation of the Wisconsin drift), it is interesting to quote Wright and Upham's[1] dictum that "the late divisions of the glacial period were far shorter than its Kansan, Aftonian and Iowan stages," and the estimate of Chamberlin[2] that makes the ratio of time to the present from the earliest Wisconsin and from the Kansan stages as 2½ and 15 respectively, leaving an undetermined figure for the still earlier portion of the glacial period. That would make, according to Taylor's figuring of 150,000 to 300,000 years for the Wisconsin invasion and retreat, a period of somewhere between 900,000 and 1,800,000 years back to the beginning of the Kansan drift. These figures are, it is true, rather staggering, but it is not absolutely necessary to accept the two estimates and combine them. There may be other ways of reckoning the duration of the separate stages of the drift. Certain it is at least that neither of these authors is to be held responsible for the estimates of the other, or the combination of the two.

In conclusion, the reasoning of this article may be summarized as follows:

The estimates of the duration of the glacial period by prominent geologists vary almost as widely as possible. It is probably useless to attempt to obtain any approximate estimate of its maximum duration, but we have in the transportation of erratics a simple method by which an ultimate minimum of the time occupied may be obtained. Accepting the land ice hypothesis of the deposition of the till, we must from all analogies, and all our knowledge of glaciers and ice-caps, admit that the motion of the ice-sheet was slow, and that it probably did not exceed a few inches a day; indeed, apart from the evidence of the till and its contained erratics, it is hard to find any grounds for belief in its motion over large proportions of the occupied territory. Erratics

[1] Loc. cit., p. 360.

[2] Loc. cit.

that are known to have been transported distances of from 500 to 1000 miles could not have traveled faster than the main body of the ice, and must, as we know from the evidences they bear of retardation and friction, have traveled much more slowly. Even allowing the extravagant estimate of two feet *per diem* for the ice movement throughout (and the recent investigations of Chamberlin and others on the Greenland ice-cap have demonstrated that this is an improbability) we can demonstrate that a single invasion competent for the transportation of a single erratic from its northern source to the southern limits of the drift would have required a period of from 15,000 to 20,000 years.[1] If we admit, as is more reasonable, that the average ice motion was much less than this,—probably not over a very few inches *per diem*, we will have to more than quadruple this estimate. Taking into account, however, the inevitable conclusion that the duration of a single ice invasion was not limited to the conveyance of a single erratic or simultaneous group of erratics, and that there were, in all probability, several of these invasions with intermediate periods of sufficient length to allow the development of extensive forests and the accumulation of heavy deposits of vegetable mold, indicating a lapse of probably many thousand years, we are compelled to multiply the above figures by an indefinite multiplier. The outcome in any case is that the brief duration allowed for the glacial period by some recent authorities is absolutely incompatible with the evidence of erratics, according to the land ice or glacier theory of the deposition of the drift.

[1] I have not in my argument taken account of the slowness of advance of the ice border which must also be considered in calculating the total. Opposed to each year's advance there must have been a summer's melting, and judging from the evidence of the Greenland ice-cap, this latter element must have been of considerable importance. The Greenland ice-sheet hardly gains at all upon the unoccupied land even in North Greenland, the ice surplus all escaping by the few glacial outlets which are much less active than those in South Greenland. In the case of the Laurentide glacier trenching upon a fairly temperate region with a long summer, the estival melting must have been quite marked. The additional transportation by flood torrents, etc., can be estimated by the extent of the till deposits as compared with glacial striæ, which even in southern Illinois are reported as found on the underlying rocks nearly to the southern margin of the drift.

I have not attempted in this article to exhaustively discuss the evidence of the slow motion of the ice in the Laurentide glacier. Much more could have been said on that point, but anyone who has followed the recently published studies of glacial phenomena in Greenland by Chamberlin, Salisbury, and others, will be able to supply most of the deficiencies in my argument. The present paper is simply the statement of views that were suggested by a consideration of some aspects of the glacial theory as an amateur geologist.

I wish to also acknowledge here my indebtedness to Professor T. C. Chamberlin for valuable suggestions on certain points here discussed.

H. M. BANNISTER, M.D.

CHICAGO.

ON THE PRESENCE OF PROBLEMATIC FOSSIL MEDUSÆ IN THE NIAGARA LIMESTONE OF NORTHERN ILLINOIS.[1]

For more than a year past my attention has been directed to some peculiar fossils from the dolomitic Niagara limestone of northern Illinois, in the collections of Walker Museum at the University of Chicago and of the Chicago Academy of Science. The exact horizon and location from which the specimens were obtained have in no case been recorded, but the general locality for them all is Joliet, Ill. Additional specimens have recently been secured from the excavations of the Chicago drainage canal, by Mr. L. H. Hyde, of Joliet, and these have been kindly loaned for study.

The complete specimens of this peculiar fossil are disk-like impressions, subcircular in general outline, with the periphery lobed, and with the surface radiately corrugated or smooth. In the center of the disk is a funnel-shaped depression and from the center of this depression a stemlike process rises to about the general level of the surface of the disk. The disk is divided by four ridges, radiating at right angles from the center, into four quadrants. The entire disk is rarely preserved intact, the larger number of specimens being the separate triangular quadrants. Several species are represented in the collection which differ in

[1] Just as this paper is going to press additional material from the collection of Mr. E. E. Teller, of Milwaukee, Wis., has come to the writer, which throws a very different light upon the nature of *Cryptodiscus*. One of the specimens in this collection shows a complete disk of *Cryptodiscus* situated at the summit of a tube composed of plates which are arranged essentially as the plates in the anal tube of *Callicrinus*. The evidence of these specimens seems to establish the fact that *Cryptodiscus* is a remarkable disk-like expansion of the four plates forming the terminal ring of the anal tube of some crinoid, probably *Callicrinus*. Additional evidence for the correlation of *Cryptodiscus* with *Callicrinus* is found in the fact that the genus *Callicrinus* occurs at every locality where Cryptodiscus has been observed. The specimens in Mr. Teller's collection will be illustrated and more fully described at another time.

the lobing of the periphery and in the ornamentation of the disk. As in the case of most of the fossils in this formation, the actual substance has been dissolved out leaving a mold in the rock, and the specimens which have been collected are generally but one side of a thin disklike cavity. None of the specimens have been actually observed *in situ*, but it is believed that the specimens usually collected, viz., those with the central depression and elevation, are the lower sides of such cavities. The upper sides are not so striking in appearance, and have usually not been preserved by collectors. A few have been observed, however, and they differ from the lower sides in being nearly plane over the central part, the funnel-shaped central depression with the central stemlike process being absent. Figure *A* represents diagramatically a cross-section of the fossil, cutting the disk diametrically.

FIG. A. *pp* points upon the periphery of the disk. *f* the funnel-shaped depression in the center of the lower side. *s* the stemlike elevation in the center of the funnel-shaped depression.

In examining the literature, two references have been found to fossils similar to those under consideration. The first of these is in the Twentieth Report of the Regents of the New York State Cabinet of Natural History, where Dr. James Hall, on Plate XI, Fig. 18, gives an illustration of a similar fossil, also from the Niagara limestone. No description of the specimen is published further than the note in the explanations of the plate, which is as follows: "The calyx of a Crinoidean? of a new and peculiar type, for which I suggest the name Cryptodiscus." No locality for the specimen is given, but the species in the accompanying paper are all from southeastern Wisconsin or northern Illinois.

At a more recent date, in the Eighteenth Report on the Geol-

ogy and Natural History of Indiana, Mr. S. A. Miller has illustrated a single quadrant of a similar fossil from the Niagara limestone at St. Paul, Ind. A short description is published, but no name is given to it. Mr. Miller states in his description that he has seen a similar fossil from the Niagara limestone near Chicago, circular in outline, and made up of four such segments as he illustrates from St. Paul. The fossil he had in mind is doubtless one of those illustrated in the present paper.

Since the fossil to which Hall gave the name *Cryptodiscus* is without doubt a form similar to those under discussion, his name will be used in their description, although it has never been properly published.

No satisfactory explanation of the nature of *Cryptodiscus* has been given by either Hall or Miller. Hall's statement with a query, that it is the calyx of a crinoidean can hardly be correct as the quadrangular symmetry is unlike that of the corresponding part of any crinoid. Miller states that some collectors have considered the fossil to be the operculum of a coral, but he himself does not seem to regard this interpretation of it as the correct one. The genus *Goniophyllum* is the only coral possessing an operculum of four triangular plates, but *Cryptodiscus*, from the configuration of the disk, need not be compared with the operculum of *Goniophyllum*.

After a careful consideration I am led to believe that these peculiar fossils may be the remains of medusæ. Though the modern jellyfish are often of large size, they are singularly unfitted for preservation as fossils because of the entire absence of hard parts. Under favorable conditions, however, impressions of these organisms have been preserved which admit of systematic determination. Such impressions have been described from the Upper Jurassic and the Upper Cretaceous in Europe. Of a more questionable nature are the Cambrian fossils from Sweden and America which have been referred to the Medusæ by Nathorst and Walcott.

Although *Cryptodiscus* has in general the symmetry of the medusæ, the idea of its being the mere impression of such a

creature is abandoned for two reasons: First, the specimens are not mere impressions in the rock, but are cavities with an upper and a lower side, from which the actual solid substance of some fossil has been dissolved. In the second place there is no reason why the mere impression in the mud of the umbrella of medusæ, should generally break into regular quadrants and be preserved as such. If we consider, however, that the specimens of *Cryptodiscus* are the casts of the gastric cavities of medusæ. both of these difficulties are eliminated. The gastric cavities of some living medusæ are divided by thin partitions into four pouches.[1] If such a gastric cavity were filled with a fine sediment, and if after the decomposition of the soft parts of the creature, this cast should become fossilized, it is easy to imagine that the four lobes of the cast might often become partially or wholly separated or destroyed during the process. The one serious objection to this theory is that probably the material originally filling the cavity of the medusa would be so nearly identical with that in which the cast was buried, that it would not be leached out, as would the shell of a brachiopod, for instance, which was of a different character from the matrix in which it was buried.

DESCRIPTION OF SPECIES.

In reading these descriptions and examining the illustrations, it should be kept in mind that they have all been drawn from the impressions in the limestone and are consequently the reverse of what the actual fossil would be. That is, what is described as a groove in these impressions would be a ridge in the actual fossil.

Cryptodiscus corrugatus n. sp. Figs. 1–2.

Disk 7 to 10 ^{cm} in diameter, finely and deeply lobed on the periphery. Funnel-shaped depression in the center 10 to 15^{mm} in diameter on the plane of the disk, narrowing below to the base of the central stemlike process which is from 3 to 5^{mm} across and rises 6 to 8^{mm} to the general plane of the disk. Sur-

[1] See figure of a *Calycozoon* (*Lucernaria*), CLAUS and SEDGWICK, Text-Book of Zoölogy, Vol. I, p. 257, Fig. 197.

face of the disk ornamented with from 18 to 20 radiating corrugations on each quadrant, which extend about two-thirds of the distance from the periphery to the center. Each groove upon the corrugated surface extends to the tip of one of the narrow lobes of the periphery. Central portion of the disk within the proximal ends of the corrugations, with the exception of the central depression, plane. The whole surface of the disk, in well-preserved specimens, minutely pitted. The four ridges which divide the disk into lobes start from four angles upon the central stemlike process. These ridges upon the more weathered specimens become nearly or quite obsolete distally. The lobed periphery of the disk is rarely perfectly preserved, and in only the best-preserved specimens can the minutely pitted surface be observed.

The specimens figured are from the collection of Mr. L. H. Hyde; beside these the species is represented in the collections of Walker Museum and of the Chicago Academy of Science. It is the commonest species of the genus which has been observed, and to it may probably be referred the specimen figured by Miller from St. Paul, Ind.

Cryptodiscus hydei n. sp. Figs. 3–4.

Disk 5 to 8^{cm} in diameter, deeply lobed between the quadrants giving the entire specimens much the form a of Maltese cross. Distal margins of the lobes in no case perfectly preserved. Funnel-shaped depression in the center 10 to 12^{mm} in diameter on the plane of the disk, narrowing to the base of the central stemlike process which is 5 to 6^{mm} in diameter, rounded over the top, and rising from 5 to 7^{mm}, a little above the general plane of the disk. Surface of the disk smooth, sometimes with shallow, ill-defined radial depressions extending from the center along the lateral margins of each lobe to the distal angles. In one specimen a shallow, ill-defined radiating depression is seen extending from near the center of the distal margin towards the proximal angle. The four ridges which divide the disk into quadrants, start from four angles upon the central stemlike process and are prominent to the bottoms of the lobes of the periphery.

The specimens figured are from the collection of Mr. L. H. Hyde.

Cryptodiscus digitatus n. sp. Figs. 6–7. 5?

Only the detached quadrants of this species have been observed. Each quadrant is deeply divided into three primary lobes, the two lateral lobes being again divided in a manner not clearly shown in the specimens. Surface covered with fine pits which are coarser towards the proximal angles. Central depression narrow and deep.

In one specimen, Fig. 5, which may belong to a distinct species, the central lobe of the quadrant is deeply bifurcate, and the lateral lobes are more deeply and more divergently divided than in the type. This specimen is also of interest in showing two quadrants somewhat separated, but still holding their relative position.

The specimens figured are from the collection of Mr. L. H. Hyde.

Cryptodiscus bilobus n. sp. Fig. 8.

A single quadrant of this species has been observed, but that one is nearly perfect. The distal margin is divergently bilobed, with each lobe marked by a well defined, rounded, radiating furrow which extends nearly to the proximal angle, dividing the plane of the quadrant into three subequal triangular areas. Each of these areas is ornamented with fine striæ which diverge from the margins of the two radiating furrows. The central depression of the disk narrow and deep.

The type specimen is in the collection of Walker Museum.

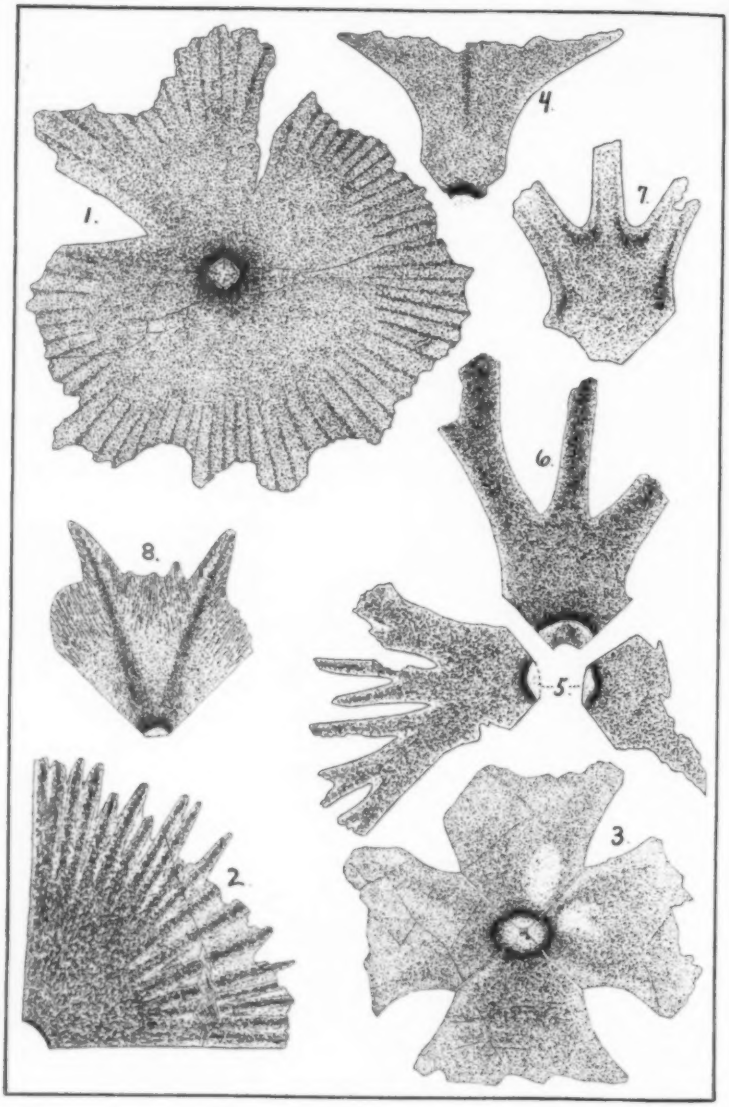
EXPLANATION OF PLATE.

FIGS. 1–2. *Cryptodiscus corrugatus.*

1. A complete specimen with the periphery imperfect.
2. A single quadrant showing some of the narrow lobes of the periphery.

FIGS. 3–4. *Cryptodiscus hydri.*

3. A complete specimen with the periphery imperfect.
4. A single detached quadrant.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FIGS. 5–7. *Cryptodiscus digitatus.*

5. Two quadrants detached but still holding their relative position. Possibly a distinct species.
6. The lower side of the type specimen.
7. The upper side of the type specimen.

FIG. 8. *Cryptodiscus bilobus.*

The type specimen.

STUART WELLER.

WALKER MUSEUM,
University of Chicago.

EDITORIAL.

THE seventh session of the International Congress of Geologists was, and undoubtedly will always remain, the most remarkable in the history of this organization. In conception and in execution its plans far exceeded those of any session that preceded it, and were much greater than may be expected for any that may follow. Governmental, industrial and social forces conspired to secure the success of the programme prepared by the Russian geologists. A most powerful government not only lent its hearty sympathy but furnished material assistance and coöperation. The Emperor and Empress received a delegation from all the countries represented at the congress, and all members in attendance were given a luncheon in the summer palace. The Grand Duke Constantine Constantinovitch and the Princess d'Oldenbourg and the Minister of Agriculture and Domains opened the session and welcomed the members to St. Petersbourg. The Grand Duke Constantine and the Grand Duchess Elisabeth Mavrikievna entertained a large number of the geologists at their palace. The mayor of St. Petersbourg invited all the members to a reception in the city hall. These tokens of good will and approval could not have been stronger; their genuineness was proved by the material benefits enjoyed by all who took part in the congress and the excursions. First-class passes were furnished over all the railroads of European Russia and Finland, good for three months, and entitling one to the use of sleeping cars. Many official courtesies were also extended which often amounted to complete freedom from customs and police surveillance, and greatly simplified traveling through various governmental provinces into remote parts of the empire. Industrial enterprises, in any way indebted to geological science, exhibited

the same energy in advancing the interests of the visiting geologists that characterizes their development of the natural resources of the country making it possible to inspect mines and study artificial exposures of great interest. The magnitude of their hospitality also will long be remembered. It was prompted by a generosity that seemed common to all classes of people throughout the empire, as was shown upon one occasion by the presentation of bread and salt by the miners of Colloubovka in token of the humble hospitality they would be glad to show us in their houses if we could have visited them. The same hearty welcome was met with in the cities and on the farms of Finland, among the miners in the Urals, and in the town, or camp, or monastery in Transcaucasia — everywhere the same generous spirit and the same expressions of good will.

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THE brilliant success of the seventh session reflects great credit on Russian geologists. To them are due both the conception and execution of the programme. The labor required for the preparation of maps and guides and for the arrangement for the meeting and the excursions can hardly be realized by anyone who has not been engaged in similar undertakings. The general secretary, Professor Tschernyschew, devoted two years to the preliminary work, and others, no doubt, had a very considerable share in it. The management of the excursions was admirable, when it is remembered what difficulties of transportation and limitations of accommodation had to be overcome and when the number of participants is taken into account. The gratitude of all the excursionists is due to the leaders of the several expeditions for the manner in which everything within their control was conducted. Their labors and good intentions were fully appreciated, except by those unfortunates whose first impulse on all such occasions is to criticise and complain, and whose subsequent effort is to find excuse for having done so. When it is remembered that in addition to their responsibility for the details of the excursion, the leaders have also to expound the geology and undergo a cross fire of questions and scientific

criticism, and not infrequently to have judgment hurriedly passed against them on insufficient evidence, the degree of indebtedness to those who undertake such responsibilities is even greater than at first appears.

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THE actual meetings of the congress were reduced to a minimum. Of the eight days set apart for the session in St. Petersbourg, four only were occupied by the reading of papers and by discussion; two were given to the opening and closing ceremonies, and two were taken for excursions. The wisdom of this allotment is open to criticism. But it was evident at the time that the most interesting feature of the session for the general member was the social intercourse between members, proving that the individuality of those present was of more immediate interest than the papers read. The audience room unfortunately was not well adapted to the purpose, owing to its large size and the interruptions by members passing through it. It was apparent to many that there would be a distinct gain if in future sessions there should be meetings by sections for those interested in specialized branches of geology, in connection with general meetings, in which all might be interested. It would permit the specialist to present more technical papers, and would allow of more time for their discussion without encroaching upon the time of others. Topics so diverse as palæontology and petrology could be treated at the same time without conflict, and with mutual advantage. The consciousness that a paper would be too technical for a general audience, and that it should be made as short as possible often deters one from devoting the necessary time to its preparation. But with the possibilities of an audience such as might be commanded at an international congress, and with time enough at one's disposal, there could be no stronger incentive for the presentation of one's best possible production. It is to be hoped that the French geologists will inaugurate this practice at the session in Paris in 1900.

The nature of the scientific proceedings of the session will be noted in another issue of the JOURNAL. J. P. I.

IT IS pleasant to announce that our colleague of the JOURNAL staff, Dr. Hans Reusch, has been engaged to give two courses of lectures at Harvard University during the current year. In the first half of the year he will treat of vulcanism, volcanoes, eruptive rocks, earthquakes and other movements of the Earth's crust. In the second half he will describe the geology of northern Europe and its relations to general geology. He will give a weekly seminar to advanced students and will take part in their field and laboratory work, his special subjects being the geology of the seashore and the geology of special districts in Europe. These lectures are given on the Sturgess-Hooper foundation recently occupied by Professor J. D. Whitney, but vacant since his death. C.

Reviews.

The Unpublished Papers of the Geological Survey of Brazil. (*Trabalhos restantes ineditos da Commissão Geologica do Brazil.*) Boletim do Musen Paraense, Vol. II, No. 2. Oct. 1897, pp. 155–204.

At the suggestion of Professor O. A. Derby, now chief of the State Geological Survey of São Paulo, the Pará Natural History Museum (*Museu Paraense*) has undertaken to publish the unpublished papers of the defunct Geological Survey of Brazil relating to the geology and physical geography of the lower Amazon. The October number of the *Boletim* contains the first installment of these papers. The parts thus published consist of an "Introduction," "The Breves Region," and "The Rio Tocantins" by Ch. Fred. Hartt, and of "The Island of Marajo" and a "Reconnaissance of the Rio Mãecurú" by O. A. Derby. These are to be followed later by other chapters on "Rio Trombetas" by Derby, on "Paracary" by Herbert H. Smith, and on the "Tajury," "Paranaquara," "Serra da Maxira," and "Monte Alegre and Ereré" by Hartt.

These papers represent work done by the extinct Commissão Geologia do Brazil from 1875 to 1878, and it might be supposed that it is now too late to publish them, especially as the Museu Paraense has lately begun active work in the same region. But it should be remembered that the State of Pará, occupying the whole of the Lower Amazon, covers an area of 443,900 square miles — nearly twice that of the state of Texas — and that the difficulties of exploration in the dense and trackless forests that cover that sparsely inhabited region are almost or quite beyond the comprehension of those who have not encountered them.

As Hartt well says, when he entered the Amazon valley for the first time in 1870, it was, geologically, a *terra incognita*. Since that time and as the result of the tireless efforts of Hartt and Derby a vast amount of important information has been gathered and published

upon the geology of the Amazon valley. Among these contributions are Hartt's and Rathbun's papers on the Devonian fossils of Pará, Derby's papers on the Carboniferous and on the Physical Geography of the Lower Amazon, and Clarke's report on the Ereré tribolites, besides a number of papers of minor importance, but all of them of value.

Director Goeldi deserves great credit for bringing out at last the work of the men who have done so much and such important pioneer work for geology in Brazil.

The Devonian fauna of the Red Maecurú. By DR. F. KATZER.

The same number of the *Boletim* contains an interesting paper by Dr. Friederich Katzer on "The Devonian fauna of the Rio Mãecurú, and its relations to the faunas of the other Devonian terranes of the globe." His studies are based upon the materials gathered by Hartt and Derby and some later collections made in 1896. The conclusion is reached that the Rio Mãecurú fauna resembles more closely that of the middle Devonian of North America than it does the lower Devonian with which it has hitherto been correlated. One of the beds he correlates more exactly with the Hamilton of the New York section. In comparing the fauna with the Devonian of Europe he says it should be compared to the upper part of the lower Devonian. "But as there can be no doubt that the Rio Mãecurú fauna corresponds to that of the Hamilton of North America, which is now considered to belong to the middle Devonian we are obliged to assume *a non-simultaneous development of certain forms in the American and European provinces of the Devonian sea, or a migration of these forms from the latter to the former provinces.* Thus the spirifers with long wings show their principal development in the Rhenish Lower Devonian, but in North America and on the Rio Mãecurú only in the middle Devonian. *Fropidoleptus carinatus* Conrad is found on the Rhine in the lower Coblenz beds, while in America, including the Rio Mãecurú territory, it occurs only in the middle Devonian. The same is true of corals of the genus *Pleurodictyum* which, in Europe, are found predominating in the lower Devonian and in America in the middle Devonian.

"All this shows that these groups of animals, probably on account of progressive alterations, especially of depth, in the sea of the first

Devonian epoch, migrated from Central Europe to America where they are now presented in the middle Devonian."

Dr. Katzer's study of these Brazilian fossils is especially interesting in connection with the work of Dr. Henry S. Williams on the fauna of the Cuboides Zone. (Bull. G. S. A. I. 481–500.)

JOHN C. BRANNER.

Report of the United States Deep Waterways Commission. By the Commissioners JAMES B. ANGELL, JOHN F. RUSSELL, LYMAN E. COOLEY. Washington, 1897.

The Deep Waterways Commission was appointed by the President in response to a joint resolution of Congress, introduced in February 1895, to make inquiry and report after conference with such similar Commissioners as might be appointed on behalf of Great Britain or the Dominion of Canada, concerning the feasibility of the construction of canals which will enable vessels engaged in ocean commerce to pass into the Great Lakes. The United States Commissioners and also those appointed by the Canadian government have devoted a year or more to the investigation and have prosecuted their inquiries with such thoroughness that their report contains much of value to geologists and hydrographers as well as the commercial world. It embraces 263 pages of descriptive and statistical matter and an elaborate series of maps, diagrams and profile sections.

Of interest to geologists and hydrographers are the tables and diagrams exhibiting the fluctuations in the levels of the Great Lakes and their outlets for each month from 1860 to 1895; a report and diagrams setting forth the effects of gales on Lake Erie; and an accurate map of the basin of the Great Lakes. The length of the ice season is treated with great fullness, there being 176 specific tables and five diagrams, covering not only the basin of the Great Lakes but much surrounding territory. The profiles setting forth the variations in depth of the several lakes with their connecting channels and of the St. Lawrence and Hudson rivers, give a clearer impression than can be obtained from charts. The great inequalities in depth found in the lower portions of the Hudson and St. Lawrence rivers are brought out with especial clearness, and they will stimulate inquiry into the history or mode of development of such abnormal stream beds.

The leading deductions from the work of the commission are as follows: First, that it is entirely feasible to construct canals between the several Great Lakes and the seaboard which will be adequate to any scale of navigation that may be desired; second, the most eligible route from the heads of Lakes Michigan and Superior is through the several Great Lakes and their intermediate channels, together with a proposed ship canal from Tonewanda to Olcott in Lake Ontario, from which the Canadian seaboard may be reached by way of the St. Lawrence River, and the American seaboard may be reached by way of the St. Lawrence River, Lake Champlain, and the Hudson River, or by way of the Oswego-Oneida-Mohawk Valley, and the Hudson River. The direct line through Georgian Bay, Lake Nipissing, Mattawa, and Ottawa rivers, although presenting no great engineering difficulties, is not considered an available alternative to the route by way of Lake Erie, since the work of construction is much more serious, the water supply limited, the ice season longer, and the amount of traffic along the line much smaller. Until comprehensive surveys have been made it will be impossible to say how far lockage and restricted channels will offset the apparent saving in distance. F. L.

The Former Extension of the Appalachians across Mississippi, Louisiana and Texas. By PROFESSOR J. C. BRANNER. From the *American Journal of Science*, Vol. IV, November 1897.

The paper is a brief and compact statement of the ground upon which the author concludes that the Appalachian Mountains formerly had the extension indicated by the title. That the mountains disappeared by subsidence over the area named is evidenced by the following: (1) the reversal of the drainage of both the Arkansas and the Texas Carboniferous areas; (2) the truncation of the eastern part of the Ouachita uplift by Cretaceous and Tertiary sediments; (3) the general slope of the Ouachita uplift is toward the east; (4) the general direction of the drainage of the Ouachita uplift is toward the southeast, which is the direction of the principal axis of disturbance; (5) the faults and folds across the eastern end of the Boston Mountains are approximately parallel to the Cretaceous and Tertiary margin; (6) the great fault near the Tertiary border of Texas and the still greater faults in Alabama, with the downthrow (which is great) on the embayment side of

the Appalachian axis; (7) the eruptive rocks and hot springs accompanying the faults and Tertiary border in Texas and Arkansas; (8) the great thickness (5000 to 10,000 feet) of the Cretaceous and post-Cretaceous sediments in the depressed area.

Among other important things, the author concludes that the Ouachita uplift is the structural equivalent of the Cincinnati-Nashville arch; that the Coal Measure drainage of the Illinois-Indiana-Kentucky area was into the Carboniferous mediterranean sea through the Arkansas valley; and that the drainage of the Arkansas and Texas Carboniferous areas was reversed about the close of Jurassic times, when the orographic movements to the east submerged the Appalachians in Mississipi, Louisiana and Texas.

The Palæozoic sediments on the south side of the Ouachita uplift are coarser than on the north side, indicating that they came from the the south. The same change of sediments is seen in the Silurian novaculites of the Ouachita uplift. It is on this ground that the Ouachita uplift is made the equivalent of the Cincinnati arch.

A. H. PURDUE.

ARKANSAS STATE UNIVERSITY.

Maryland Geological Survey, Vol. I. WM. BULLOCK CLARK, State Geologist. The Johns Hopkins Press, Baltimore, Md.

Following the good example set by some of the recent state geological surveys, the survey of Maryland presents in its first published volume a summary of the geological work which has already been done within the state. This ground is covered in Parts II, III and IV of the present volume, each of which treats of the subject from a different point of view. The first gives a history of the various organizations which have carried on geological work within the state, and references to the work of individuals not immediately connected with organizations. The next presents a summary of existing knowledge concerning the geology of the state, unencumbered by references to the men who did the work, the dates at which their results became known, and the publications where they were set forth, references which, if present, would seriously interrupt the continuity of the sketch. In this sketch are incorporated some of the results of the reconnaissance work of Dr. Clark and his assistants since the organiza-

tion of the present survey. The third part of the report referred to above (by Dr. E. B. Mathews), is a careful bibliography of the publications, both textual and cartographic, touching the geology and natural resources of the state. These chapters, which constitute the strictly geological part of the volume, are prefaced by a chapter which sets forth the plans and purposes of the survey — a chapter well worth the perusal of those who are charged with the organization or execution of such surveys.

Part V is a report by Dr. L. A. Bauer on the magnetic work in Maryland, and includes a sketch of the history and objects of magnetic surveys. Dr. Bauer has determined the magnetic elements at a number of points, and has brought together all data which are now known concerning this interesting subject, so far as applied to the state.

The volume is illustrated by seventeen well-executed plates and maps, among which are a geological map of the state as now understood, an isogonic map, and a map showing lines of equal magnetic inclination and the preliminary lines of equal magnetic force (for January 1897).

The volume is to be commended not only for its contents, but for the excellence of its typographic work. In this respect it is in pleasant contrast with the cheap volumes sometimes issued by similar organizations. R. D. S.

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